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THE PAST AND PRESENT STATUS OF THE ETHER[1]

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IN a recent letter to the New York *Nation,* Professor William James, in describing the philosophy of M. Emile Boutroux, makes the statement that "theories result from psychological variations, just as Roosevelts and Rockefellers result from biological variations." Of the entities of science he says:

> The creative touch of human reason was needed in each case for the extrication; and that those particular creations resulted rather than a hundred others just as possible, is one of those selective interactions between living minds and their environment which can be "understood" when once it has occurred, but which no acquaintance with the previous conditions can show to an outsider that it was the sole thing possible.

Considering the prevalence of such philosophical views, and the fact that many persons believe that physics is now undergoing a sort of crisis, in which many of our most cherished ideas are about to be relegated to the scrap-heap, I believe it to be not without profit to consider the past and present condition of our views with regard to the luminiferous ether, and to cautiously forecast their future.

Certainly the postulate of the existence of the ether has been until very recently one of the fundamentals of physics (including astronomy). At the congresses of arts and sciences held at St. Louis in 1904, the subject of physics was, like all Gaul, divided into three parts, physics of matter, physics of ether, physics of the electron, and although this division was, I believe, not made by a physicist, this must have made little difference. In an interesting book published less than a year ago by Sir Oliver Lodge, entitled "The Ether of Space," the properties of the ether are set forth with a concreteness and dogmatic manner that is now becoming unfashionable, and relieves that writer

[1] Read at a meeting of the American Philosophical Society, April 22, 1910.

from any suspicion of being called a pragmatist. For him the ether undoubtedly is a real thing. In a more ambitious treatise published ten years ago, by Sir Joseph Larmor, entitled "Ether and Matter," we have a thoroughgoing mathematical investigation of the properties of the ether, and, as the subtitle states, a development of the dynamical relations of the ether to material systems. And yet, since the publication of the latter work there have been voices heard with ever-growing distinctness, declaring in not dubious terms the lack of necessity of any such conception as that of the ether, and threatening the belief in its existence with relegation to the company of phlogiston in the morgue of dead theories. That we can not dismiss such voices with contempt is evident if among them are to be counted those of such leaders of physical science as Henri Poincaré, Sir J. J. Thomson and Professor Max Planck.

Before we can discuss the question of the existence of the ether, we must first determine what we mean by that term. This is undoubtedly the main difficulty with the whole matter. The article in the "Encyclopædia Britannica," written over thirty years ago by Maxwell, as competent an authority as could have been named at that time, begins with the definition, "a material substance of a more subtle kind than visible bodies, supposed to exist in those parts of space which are apparently empty," and ends with the statement, "Whatever difficulties we may have in forming a consistent idea of the constitution of the ether, there can be no doubt that the interplanetary and interstellar spaces are not empty, but are occupied by a material substance or body, which is certainly the largest, and probably the most uniform body of which we have any knowledge." This is certainly flat-footed enough, but how different from the conclusions of Lodge, one of the present survivors of the same school, we may see from his book above mentioned.

The need for the idea of an ether is well shown by the following quotation from Newton, who, after describing an experiment of two thermometers, one in a vessel filled with air and the other in vacuo, being carried from a cold place into a warm one, both rising at the same rate, says:

> Is not the heat of the warm Room conveyed through the Vacuum by the Vibrations of a much subtiler Medium than Air, which after the Air was drawn out remained in the Vacuum? And is not this Medium the same with that Medium by which Light is transmitted, and by whose Vibrations Light communicates Heat to Bodies?

And yet Newton did not accept the wave theory, but by the influence of his great name bolstered up the emission theory for a hundred years. It was his contemporary Huygens, who must be credited with the invention of the ether in order to explain the propagation of light. Huygens's ideas of the properties of the ether were, however, very dif-

ferent from those that have now been held for a century. In order to cover all the different notions that have been held, without being so definite in making the ether a substance as was Maxwell, we need only ask the question, Since we know that light travels with a speed of about three hundred thousand kilometers per second, and takes about eight minutes to come from the sun, what is the state of the light after it has left the sun and before it has reached the earth? We reply, it is traveling through the ether. A similar definition was given by the late Lord Salisbury who said that the noun ether was the subject of the verb to undulate. But why undulations? The undulatory theory, as a successful explanation of optical phenomena, is just about a century old, and was propounded by Dr. Thomas Young, in two Bakerian lectures before the Royal Society in 1801 and 1803. The reason that convinced Young, and later the scientific world, of the undulatory nature of light, was the fact of interference, or the production of darkness by the simultaneous action of two beams of light, carefully investigated by Young. These views were savagely assailed by Lord Brougham, in a scurrilous article in the *Edinburgh Review,* in which he says that "it is a metaphysical absurdity, to assert that qualities can move in concentric surfaces." The violence of the attack may be seen from the quotation:

> The long silence which he (Young) has since preserved on philosophical matters, led us to flatter ourselves, either that he had discontinued his fruitless chase after hypotheses, or that the Society had remitted his effusions to the more appropriate audience of both sexes which throngs around the chairs of the Royal Institution.

It is evident that Young had an excellent understanding of the analogy between sound and light waves, but he did not follow out the theory with the mathematical exactness bestowed upon it by Augustin Fresnel, whose superb researches, beginning in 1815, have made his name a classic of optical investigation. Both Young and Fresnel recognized, as Huygens had not, the fundamental difference in the nature of waves of light and sound, namely, that since by turning the proper apparatus traversed by light about the direction of the beam as an axis, the light is capable of alternate extinction and transmission, the undulations must be transverse to the direction of propagation. Fresnel introduced into his mathematical treatment certain mechanical principles, notably that one which we now call the conservation of energy, but he did not attempt to find a mechanical structure, in terms of properties of ordinary matter inertia and rigidity, which would explain the nature of the ether. This was done by George Green, who assimilated the ether to an elastic solid, which is capable of transmitting transverse waves in all directions with the same velocity. Unfortunately, such a solid transmits equally well longitudinal waves, like those

of sound, but with a different velocity from that of transverse waves. But such longitudinal waves have no place in any optical phenomenon, and therefore constitute a difficulty for the theory. In order not to have them it was necessary for Green to suppose the ether incompressible. Thus the theory did very well for the propagation of light on free space. When light passes from free space to a transparent substance, however, it is partially reflected and partially refracted, travelling with a different velocity in the new medium. This change of velocity could be explained by a difference of either density or rigidity in the two media. Green chose one hypothesis, in fact the same as that of Fresnel, Neumann and McCullagh the other. This difference gave rise to a controversy over the direction of the vibration, as to whether it was in or perpendicular to the plane of polarization, a controversy vainly sought to be settled by experiment. Although reflection and refraction could thus be explained, there remained a very grave difficulty. The conditions to be satisfied at the surface between two different media are too many to be satisfied by a transverse wave alone, so that had there been originally only a transverse wave, it would give rise to a longitudinal wave on striking the surface limiting the media. To avoid this difficulty a mechanical theory was proposed by McCullagh, in which the elasticity was not like that found in any known substance, but was called into play when a portion of the medium was rotated, quite independently of whether neighboring portions were rotated or not. This theory gave a very satisfactory explanation of reflection and refraction, but long met with opposition on account of its postulating elastic properties not found in any substance.

Probably the person who took most seriously the view of the ether as having the properties of some familiar sort of matter was Lord Kelvin, who devoted a large portion of his life to the attempt to find a suitable mechanical representation of the ether. In fact he stated on the occasion of his jubilee that for forty years this question had not been absent from his mind for a single day. Lord Kelvin frequently uses the term "jelly" as typical of Green's elastic substance, and did finally, by a very ingenious assumption, succeed in assimilating the ether to such a substance. But in spite of all these attempts, we may agree with the opinion of Lord Rayleigh, who concludes that for many reasons "the elastic solid theory, valuable as a piece of purely dynamical reasoning, and probably not without mathematical analogy to the truth, can in optics be regarded only as an illustration."

Such was the condition of affairs at the close of what I may call the medieval period in optics, when, in 1864, Maxwell gave affairs an entirely new turn by the presentation of his famous paper on "A Dynamical Theory of the Electromagnetic Field." In this he was guided by the conjecture of Faraday that the same medium which is concerned

in the propagation of light might also be the agent in electromagnetic phenomena. Faraday says:

> For my own part, considering the relation of a vacuum to the magnetic force, and the general character of magnetic phenomena external to the magnet, I am much more inclined to the notion that in the transmission of the force there is such an action, external to the magnet, than that the effects are merely attraction and repulsion at a distance. Such an action may be a function of the ether, for if there be an ether, it should have other uses than simply the conveyance of radiation.

This expression of Faraday is the key-note of Maxwell's theory. In examining the properties of the medium necessary to transmit electric and magnetic forces, he concentrates his attention on two quantities having direction, namely, the magnetic and electric polarization of the medium at every point. He shows that these states of polarization are propagated in waves, and that these waves have all the properties of light-waves. They are transverse, no longitudinal wave occurs, and moreover for the first time the conditions at the surface of separation of two media are exactly sufficient to give the proper explanation of reflection and refraction. Everything accomplished by *any* undulatory theory was accomplished by the electromagnetic theory, with this in addition, so that it is perhaps surprising that it remained for the experimental production in 1888 by Hertz of undoubtedly electromagnetic waves having all the properties predicted by Maxwell to give this theory the overwhelming preponderance that it has since maintained.

We may now touch upon the question, what is a mechanical theory. A mechanical theory is one that can be stated in terms of the principles of mechanics. The laws of mechanics, as they have been held since their exact statement by Newton, are all embraced in the single mathematical principle of least action, best comprised in the enunciation of Hamilton. In this enunciation occur two functions representing the two forms of energy, kinetic and potential. If these depend in a certain simple manner on two quantities having direction, or vectors, irrespective of their physical nature, the differential equations follow, which lead to wave propagation. Maxwell's field vectors have this property, and consequently Maxwell's theory *is* a mechanical theory. I will now define the properties of the ether, as they seem to me to be required by our present-day notions. The ether connotes those properties of space in virtue of which a change in either of two field vectors at any point gives rise to a field of the other sort, the lines of which tend to symmetrically surround the lines of the original and varying vector in circles. In addition the direction of these surrounding lines is contrary according to the field that we begin with. This is a qualitative statement in plain English of what is quantitatively stated in the six differential equations of Maxwell's theory, and it avoids the use of the

electromagnetic terminology. It thus applies exactly to Fitzgerald's and Larmor's resuscitation of McCullagh's rotational elastic theory, which is found to be identical with the electromagnetic theory.

I believe that I have thus given that definition of the ether which best agrees with what Boltzmann calls the phenomenological view in physics which attempts to exactly describe phenomena, without any hypothesis, or any attempt at mechanical model to assist the imagination. This was the view of Kirchhoff, Helmholtz, Hertz and Boltzmann, and I believe it to be the most scientific. The English method, of which Lord Kelvin was the leading example, demands concrete models, which resemble the phenomena more or less, and which are frequently changed. In the words of an acute French critic, M. Duhem, for a geometer of the school of Laplace or Ampère, it would be absurd to give for the same law two theoretical explanations and to maintain that the two explanations hold simultaneously; for a physicist of the school of Kelvin or Maxwell, there is no contradiction in the same law being represented by two different models. I may also quote Fitzgerald's words:

> I can not conclude without protesting strongly against Sir William Thomson's speaking of the ether as like a jelly. It is in some respects analogous to one, but we certainly know a great deal too little about it to say that it is like one. I also think that Sir William Thomson, notwithstanding his guarded statements on the subject, is lending his overwhelming authority to a view of the ether which is not justified by our present knowledge, and which may lead to the same unfortunate results in delaying the progress of science as arose from Sir Isaac Newton's equally guarded advocacy of the corpuscular theory of optics.

I feel that this protest is a very mild one, and that the attempt made by Kelvin to determine the density and elasticity of the ether, from very questionable assumptions, together with the recent attempts of Lodge, based on equally naïve conceptions of the nature of the ether as a concrete substance, are greatly to be deplored.

We come now to the most modern development of the ether theory. Maxwell had, as has been said, accurately described the propagation of the electromagnetic waves, and had given the differential equations governing their propagation. It remained to add to these equations terms expressing the genesis of the waves, to show how these resulted from the motion of charges of electricity. This was done in an important series of papers begun in 1892 and continued until the present by H. A. Lorentz, who may be characterized as the legitimate successor of Maxwell. Not only did Lorentz add terms shown to be necessary by the experiments of Rowland on the magnetic effect of moving electric charges, and later by the deflection of the cathode rays by a magnet, but he succeeded in showing for the first time how the potentials determining the field were propagated in time through the field, a result vainly sought by Gauss, Weber and Riemann, and almost

reached by the latter. The object of Lorentz in his papers was to explain the transmission of waves in moving media, beginning with the explanation of astronomical aberration. Singularly enough this was the one phenomenon which was better explained on the emission than on the undulatory theory, and which had proved a stumbling-block for the latter. If the ether is a substance, the question arises whether it is carried along by the earth in its motion, or whether it remains fixed. Lorentz assumed that it remains fixed, and thus satisfactorily explained aberration. But if the earth moved through the ether, the velocity of light between terrestrial points should be affected in the same way that the velocity of sound is affected by the wind. To test this a celebrated experiment was made by Michelson in 1881, repeated by Michelson and Morley in 1887, and several times later, which showed the failure of the earth's motion to influence the velocity of light from a terrestrial source. This classical experiment may prove to be the beginning of the end of the ether. It is evident that if light is propagated through the ether in waves which have a velocity peculiar to the ether, and not influenced by the velocity of the source, then light will take longer to reach a point a given distance from it when both are moving in the direction of the line joining them when the second point is ahead than when it is behind, in the ratio of the sum of the velocities of the source and the waves to their difference. The time for the light to go to the forward point and come back is greater than it would be if the system stood still by an amount inversely proportional to $1-\beta^2$ where β is the ratio of the speed of the source to that of light. In the case of the earth this is about one part in one hundred millions, and it was shown by Michelson that no such effect existed. Michelson assumed that this showed that the ether was fixed to the earth. For the contrary explanation, Lorentz adopted an hypothesis already proposed by Fitzgerald, namely, that all bodies in motion are thereby shortened in the direction of their motion, in precisely this ratio. This hypothesis, though startling, has now obtained great weight. In connection with it, Lorentz introduced the idea of local time, which is different for different points of the same system moving with a uniform velocity of translation. The modification, by the motion, of both distance and time leads to a most fundamental principle for all our physical notions, called the principle of relativity, which, though brought about by Lorentz, was most clearly expounded by Einstein, who is probably the high priest of the ultra-modern school. The principle of relativity assumes as a postulate that all phenomena are the same if observed with reference to a body moving with constant velocity with respect to the ether as if with respect to a body at rest. If this is so, and no experiments have contradicted it, we have as much right to suppose the ether at rest with respect to one body as another. It seems then unnatural to characterize one body as moving relative to a fixed ether. Hence

Einstein abandons the ether, which he declares to be the totally unnecessary conception. Einstein makes two postulates which are sufficient to explain all phenomena now known. The first has been stated, the other is that the velocity of light is the same when measured in any system. By measures of this velocity, we can, therefore, not determine whether the system is moving or at rest. Clothed in a more mathematical form, such as has been given by Minkowski, we may state the principle as follows: If instead of the distance x measured in the direction of the motion of the system, and of the time t measured by a clock standing still, we substitute a quantity x' denoting a new length and t' a new time, then all the equations of electro-dynamics and presumably all those of physics admit of a so-called linear transformation of the variables x and t to the variables x' and t'. Under this transformation, the equations remain, therefore, absolutely unchanged. It is accordingly impossible by any observations to determine whether the time measured by the clock is t or t' or whether the distance measured by the scale is x or x'. As has already been said, this proposition is of the most startling nature and results in connecting the notions of time and space in a most unexpected manner. In fact we may briefly sum up by saying that we can not tell *where* a point is until we know *when,* and we can not tell the time *when* until we know the place where! If we accept this principle it may be necessary to totally abandon the hypothesis of the ether. Certain writers, such as Ritz in France, have established a system of electrodynamics in which the conceptions of the ether and of the magnetic and electric fields have totally disappeared. Ritz, for instance, bases his whole theory upon the so-called retarded potentials of Lorentz, by means of which the action of any electric charge, fixed or in motion, is calculated at any other time and place by means of definite integrals. This conception has been vigorously maintained: in England I may mention the name of Mr. Norman Campbell, who in a recent article in the *Philosophical Magazine,* as in his excellent modern treatise on electromagnetic phenomena, has vigorously assailed and even ridiculed the school of those whom he calls the "etherealists," as making use of a totally useless and hindering conception.

In 1900 Professor Poincaré had already asked the question, "does the ether exist?" This I may characterize as now the question of the hour. To sum up what I believe to be the state of the case, certain phenomena concerning radiation and the distribution of energy in the spectrum have led to the necessity of certain assumptions which seem difficultly explained on the ether hypothesis. Sir Joseph Thomson also, in order to explain certain phenomena connected with the emission of electrons from metals under the action of ultra-violet light and other phenomena with which he is particularly competent to deal, has propounded the hypothesis that a wave of light is not uniform but is somewhat of a fibrous nature. I find it difficult to see how such a hypothesis

is to be reconciled with the hypothesis of the ether or the differential equations at all. In fact, the views of Sir Joseph are to me in many places incomprehensible. In his lectures recently delivered at the Royal Institution on the electromagnetic theory of light, however, Sir Joseph categorically expresses himself as of the opinion that the electromagnetic theory of light is one of the great achievements of modern science. To me this means that he approves of the ether. To take the extreme argument of Ritz, who employs as a fundamental necessity the retarded potential, seems to me to be exactly the same thing as to say that the ether exists, for since nothing whatever is propagated with infinite velocity, this is the same to me as saying that it is propagated in the ether. In the first part of this paper, I have defined what I mean by the ether in very guarded form. This definition I see no reason to change. Whether we begin with the retarded potential and find that it satisfies a differential equation, or whether we begin with the differential equation and find that it is satisfied by a retarded potential is to me a matter of utter indifference and implies an ether. I admit that we still have to find a hypothesis for the ether which makes it give rise to this differential equation. The hypothesis of Maxwell seems to me the easiest one yet proposed. I will therefore close by stating my present opinion, that the ether is as good to-day as it ever was, but that apparently the notions of time and space have had to be modified in the method suggested by Lorentz and splendidly developed by Einstein and Minkowski. At the same time, we can not deny that there exists to-day what we may call *la crise de l'éther,* and we are far from being able to say with Lord Kelvin, " It is absolutely certain that there is a definite dynamical theory for waves of light, to be enriched, not abolished, by electromagnetic theory."

PHYSIOLOGIC LIGHT

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THERE are probably but few if any of the readers of this magazine who have not seen and admired at least one of the many manifestations of "physiologic light," of which the most common to us is the firefly. Indeed, from the earliest times the phenomenon of the emission of light by animals and plants has attracted man's attention, and a large amount of scientific work has been done upon the subject. An attempt to compile a complete bibliography of the subject has resulted in the remarkable discovery that there are over seven hundred references to the literature bearing on the emission of light by organized bodies, and "the end is not yet." The work has embraced the physical, chemical, physiologic, histologic and entomologic sides, and much valuable information and many interesting facts have been secured. Among the names of the early writers who refer to some phase of this phenomenon are Aristotle, Pliny the Younger and Josephus; the more recent names include those of Robert Boyle, Sir Humphry Davy, Faraday, Pasteur, Kölliker, Dubois and the late S. P. Langley, and indeed a host of others whose names are more or less widely known. Several extensive treatises on the subject have appeared, some of which are really quite good, though regrettably they are for the most part out of date at this time. For the benefit of those who may care to read further, the names of a few of these are given below.[1]

The phenomenon of physiologic light has been variously termed "phosphorescence," "luminosity," "photogenic function," etc., by different authors. As these are, for the most part, interchangeable in meaning, they will be used in this paper to refer to the same thing. The term "phosphorescence" is unfortunate, since it implies that the light is due to the presence of the element phosphorus—which it is not—and has become still more objectionable recently owing to its application by physicists and chemists to another totally different phenomenon of light emission.

It was my good fortune during the summer of 1909 to be associated with Professor J. H. Kastle, of the University of Virginia (then chief

[1] Holder, C. F., "Living Lights," Scribner's, 1886, New York; Gadeau de Kerville, "Les Insectes Phosphorescents," Rouen, 1881, 1887; Gadeau de Kerville, "Les Animaux et les Vegetaux Lumineux," Paris, 1891 (German edition by Marshall, Berlin, 1893); Dubois, "Les Elaterides Lumineux," Paris, 1886; Dubois, "Physiological Light," Smithsonian Institution, Washington, D. C., Report for 1895, pp. 413–431.

of the Division of Chemistry of the Hygienic Laboratory, of the U. S. Public Health and Marine Hospital Service), in a study of the effects of various chemical agents on the emission of light by the common firefly of the country around Washington, *Photinus pyralis* K. In the progress of this work we had occasion to review the available literature quite thoroughly, and were struck with the lack of acquaintance of people generally with the theories which had been advanced to explain the phenomenon, and with the work which had already been done upon it. The results of this investigation will be published at an early date. In spite of this great amount of work which has been done, the firefly still preserves its secret of "the cheapest form of light," and seems likely to do so for some time yet.

Although the most common and brilliant manifestations of physiologic light are exhibited by the fireflies, this property is by no means confined to the animal kingdom. Various vegetable forms, from the lowest to the highest, have been reported as producing light. There are many varieties of luminous bacteria and molds, whose activity is seen in the luminous decay of fish and wood. Certain agarics and other of the higher fungi are luminous, and the light given by the underground rhizomorphs of fungus growths is among the first of these phenomena to be reported in scientific literature. Of the higher plants, the marigold, the nasturtium and other garden and wild flowers have been said to emit flashes of light—a circumstance attributed by Phipson to electricity. But, for the most part, the light of vegetable forms seems to be pale and often hard to discern, as compared with the brilliancy and glitter of the firefly and other animal forms.

To those not living on the sea-coast, the most common manifestation of the photogenic function is that produced by some variety of the firefly; but there are a large number of marine forms of varying degrees of organization which possess this property, and some of these are common on certain coasts. For the purpose of discussion, the animal forms will be grouped as marine and land forms.

The simplest marine form which emits light is the "Noctiluca" (*Noctiluca miliaris*), a tiny globule of protoplasm scarcely a millimeter in diameter, which when present—as it usually is—to the extent of millions upon millions, produces the appearance known as the "milky sea" or "phosphorescent sea." Many interesting studies have been made on this little organism, the principal importance of which lies in the fact that it seems to give practically the same reactions as other more highly organized luminous forms. Besides the Noctiluca, certain Beroe and other Ctenophores are often present in immense numbers, and give rise to the same appearance of the milky sea. Higher still, there are a number of Salpæ, and other marine forms which give light, and interesting studies upon them have been made

by Panceri, Quatrefages and other scientific men. But perhaps the most remarkable luminous marine organism is the bivalve, *Pholas dactylus,* known to the French as the "Pholade," and to the Germans as the "Bohrmuschel." This creature has definite luminous organs, whose tissue and secretions are strongly photogenic. It has been the subject of interesting researches by Dubois, and has been shown to react in a manner similar to that of other luminous forms. More recently, certain peculiar organs possessed by deep-sea fish have been determined to be light organs, and thus it appears that in the depths of the sea they need "artificial" light, when the sun's light fails to penetrate, just as on land when the sun is hid.

By far the most brilliant and most commonly known form of physiologic light is that given by the so-called fire-flies; this term embraces a large number of species of insects, mostly Coleoptera (beetles) of two or three genera. Besides these Coleoptera, there are a few luminous forms distributed among the other insects, together with certain myriapods, worms and other occasional forms. In a very few instances luminosity of more highly organized forms has been reported, but for the most part these appear questionable at least. Of the non-coleopterous insects, Diptera (*Chironomus*) and Hemiptera (Fulgoridæ) are said to be luminous; the hills of the South American termites (Neuropteræ) have also been observed to be luminous.

The majority of the insects commonly called fireflies belong to the genus Lampyridæ, including the Italian luciole (*Luciola italica*), the English and continental glow-worm (*Lampyris noctiluca*), the continental firefly (*Lampyris splendidula*), the American fireflies and "lightning-bugs" (*Photinus pyralis, Photuris pennsylvanica,* etc.), and a vast number of other luminous insects. Further south, as in Cuba, Mexico and Brazil, the more brilliant insects belong to the genus Elateridæ, and embrace the cucuyo (*Pyrophorus noctilucus*) and the cucuyana (*Pyrophorus physoderus*). In India there is said to be a luminous buprestid beetle.

Thus it will be seen that, so far from being a rare phenomenon, the emission of physiologic light is one of well-nigh universal distribution, and appears to be an important function in the life of those organisms possessing photogenic activity.

While most of the facts here given apply primarily to the fireflies, they may, in great part, be taken as true for the entire phenomenon of physiologic light. Different forms may show variations in color, intensity and mode of emission of the light, but basically it all seems to revert to the same cause—a cause as yet, however, unknown.

The light given by luminous insects is usually stated by authors to be greenish or yellowish; a few have claimed to observe insects to emit a reddish or bluish light, and marine forms have been reported to emit

a large variety of colors,—red, blue, violet, green, etc.—but the colors are in most cases pale and dim.

Perhaps a dozen investigators have submitted some form of physiologic light to analysis by the spectroscope, and with a few exceptions the results have agreed very well. The best known of these spectroscopic investigations was that of Langley and Very, in 1890. These authors worked with the Cuban cucuyo; briefly, they found that the prism of their spectroscope resolved the light into a narrow band in the yellow and green region of the spectrum, ending somewhat abruptly and showing few red or blue rays; they were unable to find that the light was accompanied by any evolution of heat, such as we ordinarily associate with light produced by combustion or by electric heating, and hence they called the paper presenting their results "The Cheapest Form of Light." This valuable research has recently been confirmed by Drs. Ives and Coblentz, working in the National Bureau of Standards, in Washington, and using more sensitive instruments than were available to Professor Langley and his coworker. Ives and Coblentz found that the light of the common firefly (*Photinus pyralis* K.), was resolved by the spectroscope into "an unsymmetrical, structureless band" in the red, yellow and green, but not extending further than wave length 0.67μ toward the red end of the spectrum, nor than wave length 0.51μ toward the violet end. From the facts at hand it seems extremely unlikely that the spectrum could be discontinuous and renewed in the infra-red or ultra-violet non-visible portions of the solar spectrum.

The remarkable fact which these researches bring out is the extremely high luminous or radiant efficiency of the light. This was estimated by Langley and Very at 100 per cent., and has been shown by Ives and Coblentz to be about 96 per cent. In other words, 96 per cent. of the total energy radiated by the firefly is exclusively illuminating radiation, and does not embrace heat or other subordinate effects. This is the more remarkable when it is considered that the best artificial illuminant has a luminous efficiency of only 4 per cent., and most of them run less than 1 per cent. Of course, this does not mean that the mechanical or chemical processes resulting in the production of the light have an equally high efficiency—that is quite another matter. But it does mean that for a given amount of radiation, the firefly produces the greatest amount of luminous radiation.

But even if we should discover the means by which the firefly produces its light, we should hardly care to use it in our homes. The insect has indeed reached the highest possible radiant efficiency, but it has been accomplished at a sacrifice of color variety that makes the light worse for color effects than even the ghastly green of the mercury vapor arc. Anything not within a very limited range of yellow and green tones would appear black.

The spectrum of the light of some of the organisms which have been reported to give reddish, bluish or other variously colored lights, is said to differ from that of the firefly.

Another very interesting fact brought out by these observers (Ives and Coblentz) is that there may be extracted from the common firefly (*Photinus pyralis* K.) a substance which is fluorescent in certain lights, and that the spectrum of the bluish fluorescent light of this substance is complementary to that of the light emitted by the insect itself —that is, the spectrum of this fluorescent light occupies that portion of the spectrum lying between the green and the violet. The presence of this fluorescent substance may, of course, be merely a coincidence; these same authors found a similar substance in a non-luminous species of the same genus, and various observers have extracted fluorescent substances from different organisms; but if it is a coincidence, it is certainly a remarkable one. Dubois has also discovered a fluorescent substance in the blood of the cucuyo (*Pyrophorus noctilucus*).

Luminous animals and their photogenic tissues are extremely sensitive to irritants, whether mechanical, electrical or chemical; in other words, these tissues are very irritable. Almost any schoolboy is familiar with the fact that pinching a firefly will result in the production of light from its luminous organ. Any other mechanical irritation, such as scratching or pricking with a pin, light taps or blows with a splinter of wood, etc., will produce a similar effect, and this is true not only of the live insect, but also of the luminous organ immediately after removal from the body of the insect; as it dries, however, the luminous organ gradually loses its sensitiveness, and when completely dry it will not respond to mechanical stimuli.

The electric current acts as a stimulus to light production. The passage of the current through the body of a firefly causes it to flash, and sea water containing the *Noctiluca* shows luminous activity during the passage of a current. Light may also act as an irritant or stimulus; Henneguy records that the admission of light to the darkened cabinet wherein were some *Noctiluca* in sea water, caused the evolution of light from these infusoria, and the local firefly has been known to flash following the turning on of an electric light in a darkened room where the insects were confined, the phenomenon being repeated several times.

The most extensive observations upon the irritability of photogenic tissue, however, have been made with chemical substances. These have included a large number of gases and vapors, acids, alkalis and salts, alkaloids, and a vast number of miscellaneous compounds. In general, chemical substances may be divided into three classes with reference to their action upon the photogenic tissue: (1) Those which tend to produce the evolution of light, and which may therefore be classed as

stimulants to light production; examples of this class are mononitrobenzene, carbon disulfide and carbon tetrachloride; (2) those substances which are neutral in their action, neither provoking luminescence nor inhibiting it; examples of this class are hydrogen and nitrogen; (3) substances which poison the tissue and permanently prevent the production of light; examples of this class are bromine, sulfur dioxide and iodine cyanide. Strychnine and other alkaloids cause the production of light, as do also certain poisons; oxygen appears to activate the production of light somewhat.

Probably the most interesting fact so far developed by the chemical study of this phenomenon is that when photogenic tissues have been dried out, the dry tissue glows again when moistened with water in the presence of air. Carradori mentioned this fact in 1808, and quoted Spallanzani and Reaumur as having made the same observation at earlier dates. Carus reported the same observation in 1864, and Dubois confirmed it some twenty years later. Professor Kastle and the writer have been able to perform the same experiment with the American firefly; it is indeed a fact that the photogenic tissue of this insect may be dried, the dry material powdered, and the dry powder kept for some time away from access of moisture, and it will, when moistened in the presence of air or oxygen, glow again; indeed, by careful redrying, the same result may be obtained two or three times on the same specimen of the dry material. Moreover, this dried tissue gives, when moistened, many of the same phenomena with chemical reagents as do the living insect and its freshly detached luminous organ. The property of thus glowing upon moistening after having been dried, does not appear to be confined to the luminous organ of the firefly, but appears to be a constant characteristic of luminous tissue as a class. The main deduction from this fact is that at least three factors are necessary for the production of light by photogenic forms—water, oxygen and some material, as yet unknown, whose oxidation in the presence of water produces light.

Several theories have been advanced from time to time to account for the production of physiologic light. Probably the earliest view was that it was due to the presence of the element phosphorus. That this is not the case is best evidenced by the fact that there are only traces of this element present in the luminous tissues, and that which is present is in the form of phosphates. Yet this is the commonly accepted view of the cause of the phenomenon, and even as recently as 1880, Jousset de Bellesme suggested that the light might be produced by the spontaneous combustion of phosphine. Carradori assumed that the luciole was capable of absorbing from the air or from its food, the "material of light," and of then emanating it again at pleasure.

The fact that the light is unaccompanied by the evolution of meas-

urable amounts of heat certainly shows that if it is indeed a combustion, it is a most remarkable one and one which differs from any analogous process known to us. The view that the light might be the result of oxidation has, however, long been held. Robert Boyle made experiments on this point in 1667, and concluded that the light produced by shining wood and fish was not affected by the absence of air, and was therefore not what we now call a combustion or oxidation. Spallanzani, as the result of his studies on luminous sea forms, came to the opposite conclusion, in which he was opposed by Macartney and Carradori. More recently this phase of the subject has been studied by Dubois, Watasé and Townsend, all of whom have published very interesting observations. As a result of these several observations the conclusion must be drawn that oxygen is essential to the process of the production of physiologic light, and that we have in this phenomenon a true but remarkable form of combustion. Of the mechanism of this process we are still very ignorant. Dubois's theory is that the light is produced as the result of the action of an "oxidase" (oxidizing ferment), to which he has given the name "Luciferase," upon a substance of unknown composition, which he calls "Luciferin," the latter being oxidized by the atmospheric oxygen through the agency of the ferment. It is a little early to accept this hypothesis finally, although it certainly presents some analogy to known processes—for example, the production of the black pigment melanin through the action of the oxidase tyrosinase upon the organic compound tyrosin. Phipson had already described a substance he called "Noctilucin" as the active principle of physiologic light; it seems possible that Phipson isolated and analyzed a culture of photogenic bacteria.

In this connection the structure of the light organs of various animal forms has been given special attention. In general, the results of studies on those forms having special photogenic organs have been essentially similar. Briefly, the luminous organs appear to be masses of cells of some special kind, possibly a fat-derivative, or according to Macaire and Kölliker, an albuminous substance penetrated by a network of trachea (tracheoles), and as the result of some chemical action, apparently oxidation, taking place in these tissues, the light is produced. Whether these tracheoles are in life filled with air or with a liquid seems doubtful; the evidence is contradictory so far as given, but it seems quite probable that they convey air.

What is the purpose of this production of light? Of what value is it to the forms which possess it? This is another side of the "secret of the firefly," which has yet to be solved. Quite probably the function bears some relation to the reproductive life of the insect. The females of the local species (*Photinus pyralis* K.) give a very much less bright light than, and are quite rare as compared with, the males; one female

to from seven to fourteen males seems to be about the proportion. The same condition appears to hold with other species of Lampyridæ also. King states that the female of the Texan form *Pleotomus pallens* is much more luminous, and rather less active than the male. In addition to the photogenic power, the common firefly is possessed of a strong and characteristic odor; Carradori also notes that the Italian luciole has an odor like that of garlic. Many insects indeed possess odors, but that of the Lampyridæ appears to be especially characteristic of the group.

In conclusion, we may say that while a vast amount of work has already been done on this interesting problem, the production of physiologic light still presents many mysteries which science has yet to explain. Nature keeps her secrets well, but this one seems well worthy of solution; the immediate practical and economic importance may not be so great as has been sometimes assumed, but it is a problem of interest alike for the physicist, the chemist, the biologist and the entomologist, and the scientific world awaits its solution with much curiosity.

INSTINCT AND INTELLIGENCE IN BIRDS—III

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I

IN earlier papers we have tried to show how the behavior of wild birds is moulded upon instinct and how some of their instincts have been modified on a large scale, or specialized in a peculiar manner. We shall now examine the other side of the shield, in order to ascertain how intelligently they work, and in relation to their intelligence it will be necessary to consider the growth of the young, and the development of certain instincts, more particularly that of fear.

Many birds, like some mammals, have been lauded by idealists, as paragons of virtue, and endowed with all the human or even angelic powers of intelligence and reason; others, again, have regarded them as the slaves of a blind or stupid instinct, whose lives are stereotyped, and run in grooves, determined largely by heredity. "Do not speak of blind instinct," says Michelet, the historian, "facts demonstrate how that clear-sighted instinct modifies itself according to surrounding conditions; in other words, how that rudimentary reason differs in its nature from the lofty human reason." "Through the thick calcareous shell, where your rude hand perceives nothing," the bird-mother "feels by a delicate tact the mysterious being which she nourishes and forms. . . . She sees it delicate and charming in its soft down of infancy, and she predicts with the vision of hope that it will be vigorous and bold, when, with outspread wings, it shall eye the sun and breast the storm."

While we are not over-zealous in applying the rule of parsimony, like most modern students, we are compelled to take a middle course. When the degrees of intelligence can be more justly weighed, the mental powers of birds, as well as of mammals, will be better understood. At present the balance does not seen to swing very far on the side of intelligence. It is certain that the instincts of birds are modified at every step by association, and that the automatism of habit is quite as striking as that due to heredity, which it sometimes replaces. Many birds learn readily from experience; some remember long, when past experience serves as guide to future conduct. It may well be doubted if they ever attain to the level of analogical reasoning, or of deliberately inventing the means in order to attain a definite aim.

Every observer is no doubt unduly influenced by the force of

isolated facts, and too often falls into temptation by trying to interpret them without a full knowledge of their history. The act in question may appear irretrievably stupid or exceptionally intelligent, while upon fuller knowledge, either view might prove wholly erroneous. Illustrations could be multiplied, but the few which follow, may be of interest.

Shrike Impaling Prey.—The great northern shrike is well known to impale its prey, such as grasshoppers, small birds and rodents, on thorns, and it presumably returns to them when in need of food, although I am not aware that the bird has ever been actually seen in the act of reclaiming its booty. According to some accounts the shrike impales its prey in order to rend it with the greater ease, but still goes on killing after it has satisfied its appetite.

On April 8 of last year I happened to witness a futile attempt at impalement under such favorable conditions of seeing all that transpired, that any mistake as to the meaning of the actions would seem to be impossible. A harsh piercing cry attracted my attention to the bird, which almost at the same moment dived into the stubble of an adjoining field, and came up with a large object in its bill. Fortunately it flew directly towards me, and alighted on the bare, lower branch of a maple tree, less than ten feet from my eye, as if completely preoccupied, and indifferent to observation. I could now see plainly that it held a little shrew, about three and a half inches long, and in a strangle grasp by the nape of the neck, for the body was as limp as a rag. The shrike at once proceeded to walk along the branch and try to impale the rodent, extending its head and drawing the body of the animal in a peculiar manner, against the soft twigs of the tree. It tried the terminal twigs, and the equally soft lateral shoots, and went through the same motions on two different branches. After several minutes of this ineffective effort, with a loud rasping call of a different character, it flew off in the direction of some woods, and was seen to descend to the ground.

The interpretation of such behavior seems obvious—that the shrike, when under the spell of a strong impulse, does not know a thorn-bush from a maple tree. Must it try tree after tree, until one of the right sort is found? If it can return to its tree by memory, why can not it find one suited to its puposes by intelligence; or, was this a bird with inherited instinct to impale, but with no previous experience with thorns?

Robin "Tying Knots."—So far as I have observed, the robin in nest-building, ties no proper knots, unless the present case (Fig. 23) be exceptional, although strings are coiled more or less effectively about adjoining twigs. This nest was placed in a crotch of a pine tree, and one of its supporting branches bore the peculiar double loop or "knot" which is here shown. It seems that a piece of string over two

feet in length was brought to the nest-site, and passed five times round the larger, and twice about the smaller of the two twigs, with overlaps due to working each string-end independently. Having thus fixed it firmly at the middle, the intelligent course would have been to have incorporated the loose ends with the nest. Instead, they were both left flying free, so that this labor, however begun, was not intelligently finished. The eighteen inches of free string really served to render the nest conspicuous.

Woodpecker Drilling for Insects.—While in the Maine woods on August 13, my attention was drawn to the freshly drilled hole of a

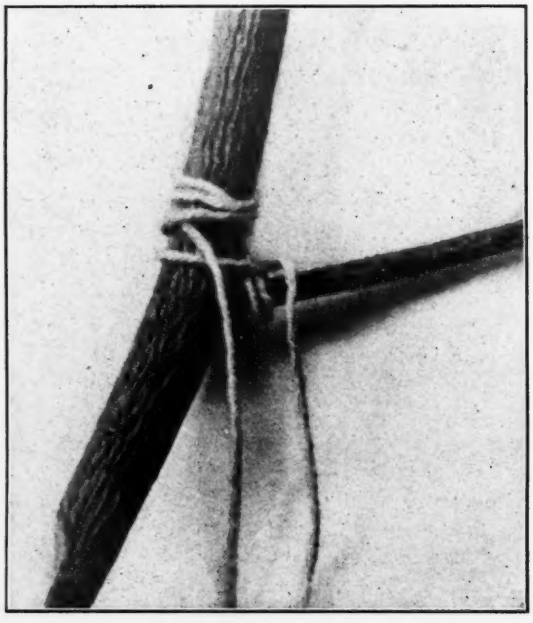

FIG. 23. DOUBLE "LOOP-KNOT" MADE BY ROBIN ABOUT PINE-BRANCH CLOSE TO ITS NEST, illustrating an act probably instinctively begun, but not intelligently finished, since the ends of the twine were not incorporated with the structure, but left hanging free.

woodpecker (Fig. 24), in a pine tree, which was two feet in diameter at the base, and apparently sound. This hole, which was remarkable for its size, had been cut at a point seven feet up, through nearly five inches of solid sap wood, to the heart of the tree, and was 9¾ inches long, 5½ inches wide, and 8 inches deep. These dimensions would imply the removal of over three hundred cubic inches of wood, and the chips, some of which were four inches long (Fig. 25), were plainly the work of our largest northern species, the pileated woodpecker or log cock.

A moment's inspection showed that this woodpecker had tunneled

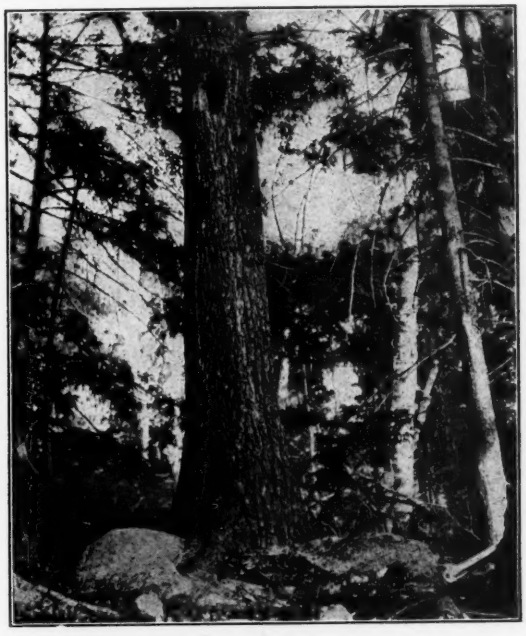

FIG. 24. HUGE BORING OF PILEATED WOODPECKER in a white pine two feet in diameter, to reach galleries of the carpenter ants, seven feet from the ground, where the insect gained entrance; illustrating either instinct to follow sounds, or intelligence in adapting means to end.

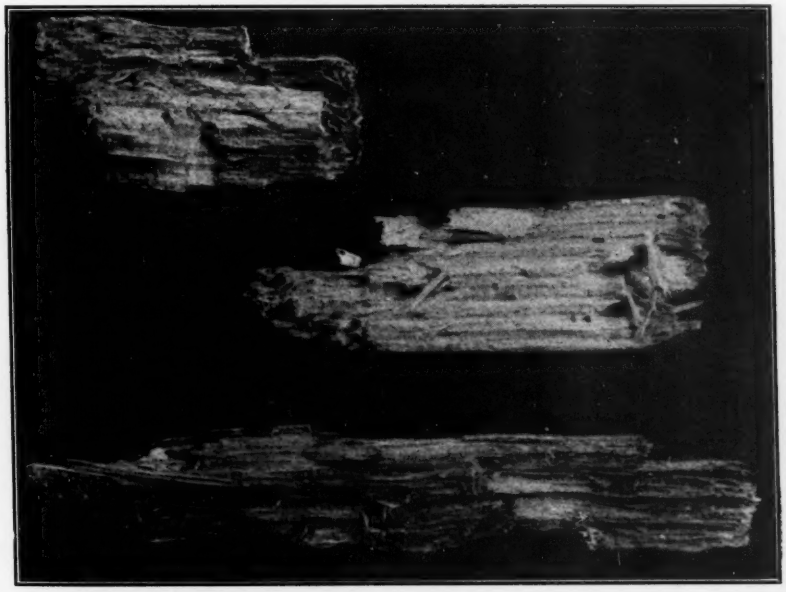

FIG. 25. SOUND PINE CHIPS FROM WORKSHOP OF PILEATED WOODPECKER, shown in Fig 24; some are bent or broken with the wrenching stroke of the bill. Natural size.

to reach the big carpenter ants (*Camponotus herculeanus*), which had extended their galleries up into the tree's heart, and some of which were already entrapped in the exuding pitch. Now one of the chief points of interest in this case was that the ant-borings were few, and the tree itself so large and solid that it would seem to be impossible for any bird to detect their presence by sounding with the bill. On the opposite side at the base, a long ant-hole was plainly seen, and at this point the observations of the woodpecker had evidently begun. A few feet above this opening it had attacked the harder wood in three different places, but desisted after making wells a few inches deep. The woodpecker then went up seven feet, where the wood was presumably softer, and made the drilling, which led to success. With these facts only known, this might appear like a case of reasoning by analogy, but there is still one sense unaccounted for, that of hearing, for the olfactory sense must be ruled out. It is possible, or even highly probable that such birds either instinctively or habitually follow the sounds in wood—sounds of wood-borers at work—and unless it could be shown that the boring of carpenter ants cannot be heard through five inches of solid wood, I should be inclined to accept this view.

This case suggests another practise of woodpeckers, the interpretation of which is clear. The president of a large university recently compared the futile efforts of certain reformers with these of a flicker which was seen to be repeatedly engaged in the vain attempt to "drill a hole through a copper gutter." The comparison may be apt to point a moral, but is a trifle unfair to both the instincts and intelligence of a useful bird, which will drum on any resonator, either to call its mates or for the pleasure of the sound, and by habit will come to the same place daily for more than a week, as in a case which we recently noticed. In this instance the resonant body was the roof of a bird house, one shingle thick, to pierce which, had that been its object, one or two blows of its strong bill would have sufficed.

II. Intelligence in Young Birds

The dawn of intelligence in young birds is seen, as we have earlier shown,[1] through the inhibition of the food-response by association—association with the parent, the nest or the vibration imparted to the tree by the touch of the parental foot.

For the first twenty-four hours, or longer, the altricious nestling behaves like a mechanical toy, and in relation to the food-response is a well-nigh perfect reaction-machine. It responds to every kind of a tactile or auditory stimulus, and within the limits of fatigue its

[1] See also "The Relation of Instinct to Intelligence in Birds," *Science*, N. S., Vol. XXVII., 1908.

responses are about as uniform and predictable as those of an electric bell. Remove the blind and naked cedarbird from its nest, and the complex food-reaction is given as regularly and as continuously as before (Fig. 26). By the second or third day, however, all this has changed, and it is difficult to get any food-response if the bird be out of its nest with which association has become established. If the young are not removed, however, the feeding reaction is usually regularly given, unless checked by satiety or the rise of the instinct of fear. Association in the early life of young birds thus tends, as we have seen,

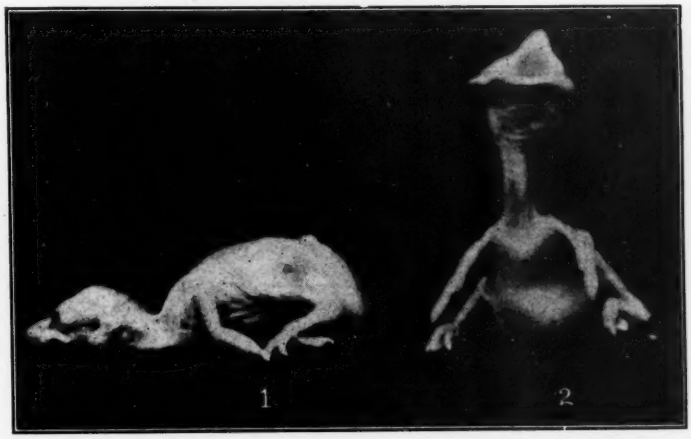

FIG. 26. YOUNG OF CEDAR WAXWING, blind and naked; but little over twenty-four hours old: *a*, typical prone position, when at rest; *b*, typical food-reaction, or reflex response to sound or contact-stimulus.

to cut out a lot of useless reactions, and to limit their responses to those which count.

Growth in Relation to the Development of the Instincts and Intelligence.—We have used the term "instinct" as synonymous with compound reflexes, that is, as reflexes involving relatively complex coordinations of the muscles and other organs. Although the sign or manifestation of an instinct may be suddenly given, the instinct itself, like every other power, seems to be unfolded gradually, and in correlation with the organs upon which its action depends.

In many precocious birds, which run, swim or fly at birth or shortly after, some of the instincts are relatively perfect at the moment of emergence from the shell, or according to certain observers even before this event, as when the young, which remain for hours with the shell chipped, are thought to respond instinctively to the warning cries of their parents. In rare cases, as in certain megapodes, they are born masters of their own destiny, and receive no care from parents which they never see. At the other extreme stand the common altrices, like the robin or cedarbird, which are blind at birth, and so helpless

that they would shortly succumb without that parental care and protection which is so faithfully rendered. Growth and development are continued after hatching, but under new conditions, and at the age of two weeks, when the nest is commonly deserted, the young so far as instincts and intelligence are concerned are at about the same stage as many of the precoces at birth.

Between these extreme types every intermediate stage is found. The American black and yellow-billed cuckoos have a place near the middle of the series, but as we have already seen, they are exceptional in many ways, not alone in the possession of great muscular strength, but in their equally remarkable muscular control, being able to grasp

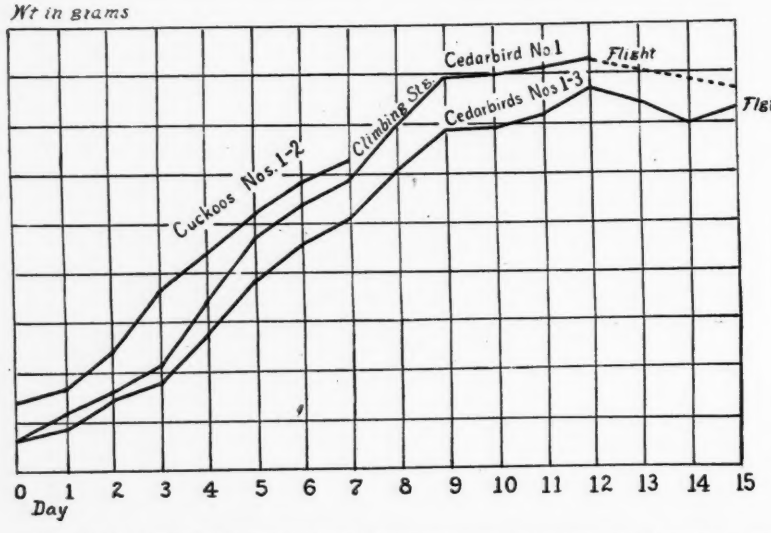

FIG. 27. GROWTH-CURVES OF THE CEDARBIRD AND BLACK-BILLED CUCKOO, as represented by the daily increase in body-weight from hatching to flight from nest, illustrating an initial stage of relatively slow growth (in cedarbird first to third day), a period of maximum increase (third to ninth day), and a final stage of retarded growth or shrinkage in weight. See table.

a twig, and with both feet pull themselves up when but four hours old, or possibly less. This ability is closely related to the climbing stage which is entered on the seventh day, when they leave the nest in succession, and ascend into the branches, where they remain for a period of two weeks before ready for flight.

In the cuckoo the curve of growth, as indicated by body-weight, appears to be quite even and regular after the beginning of the second day (Fig. 27). In these particular birds the highest rate was registered on the third day, and this proceeded without appreciable interruption until the last day in the nest, when it was slightly checked. The power of muscular coordination, association and the instincts of fear and of preening seem to develop gradually after the first day.

Thus the initial attempt to preen, which involves the complicated act of drawing the mandibles over the feather-tubes, may be witnessed on the fifth day; thereafter it is repeated more and more frequently, until on the sixth day it is an established practise, and the movements have become very precise. Gradual also is the development of fear, an early premonition of which is crouching and hugging the floor of the nest, although its final manifestations, such as bristling and spreading, giving a high-pitched alarm, or jumping out of the nest, may seem to mature suddenly, partly no doubt because the stimulus which provokes them is suddenly received.

In the altricious cedarbirds, a single family of which was weighed and measured in 1901, there was (1) an initial period of relatively slow growth, lasting three days, followed by a second period (2) of

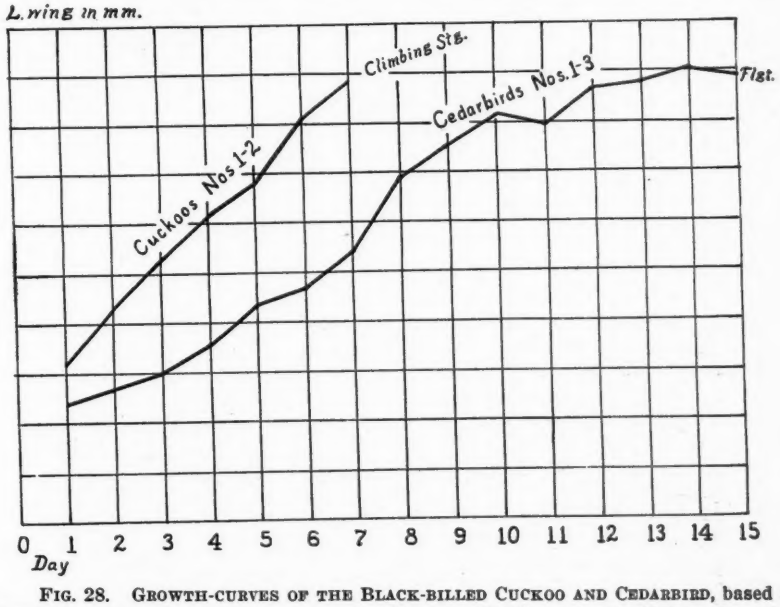

FIG. 28. GROWTH-CURVES OF THE BLACK-BILLED CUCKOO AND CEDARBIRD, based on daily increase in length of wing from hatching to climbing stage or flight. See table.

maximum increase, of six days, and a final interval (3) of fluctuating or retarded growth, extending from three to six days before flight, the birds even losing weight either before or after this event.

The growth-curve of the most vigorous member of this cedarbird family (Fig. 27), the first to hatch and to fly, is seen to start with a higher initial rate, and to maintain it from the third to the ninth day, at the age of flight. Fortunately this bird, which was then lost, was recaptured on the fifteenth day, when it is seen to have shrunk very perceptibly. It had, in fact, lost nearly three grams, or seven per cent., in body-weight. The curves showing the rate of wing-growth in both cuckoo and cedarbird (Fig. 28) follow those of body-weight very closely,

but there is possibly a variation in other organs, such as the leg or tarsus, but it is difficult to obtain reliable measurements on some of these parts. The most vigorous nestling (No. 1) more than doubled in weight on the first day, more than trebled on the second, and more than quadrupled on the third, while on the twelfth day, when it left

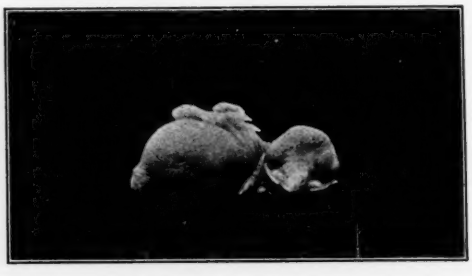

FIG. 29. YOUNG CEDARBIRD, No. 1 of table, shortly after hatching, lying on side, cicatrix of umbilicus showing on abdomen; weight 5 grams. August 10, 1901.

the nest it had increased its initial weight seventeen-fold. What ten days will do for the young cedarbird on the score of appearance may be seen by comparing Figs. 29 and 30, the first of which shows nestling No. 1 when about two hours old. The data on which these curves are based are given in the following table.

FIG. 30. THE SAME CEDARBIRD (on left) as shown in Fig. 29, ten days later with birds Nos. 2 and 3 from the same nest. All show the crouching tendency, in evidence of fear.

Kuhlmann,[2] who has recently published an interesting study of the development of the instincts and intelligence in certain altricious birds, in particular the turtle dove, the brown thrush and the red-wing blackbird, finds the rate of growth quite similar to that shown for the

[2] *The Psychological Review*, Monograph Series, No. 44, November, 1909.

GROWTH-RECORDS IN CEDARBIRDS AND CUCKOOS, FROM HATCHING TO FLIGHT, OR CLIMBING STAGES

Day.		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Cedarbird.																	
Wt. in g.	1	2.41	5.00	7.92	10.44	17.28	23.04	26.64	29.16	34.56	39.24	39.60	40.32	41.04	—	—	38.16
	2	2.41	3.42	6.48	7.92	11.52	17.28	20.16	24.12	28.08	30.90	32.76	33.48	37.44	36.36	34.20	36.72
	3	2.41	3.96	6.66	8.28	12.24	17.64	20.52	21.72	27.72	31.68	33.84	33.12	36.00	37.44	35.28	33.84
	4	2.26	2.52	3.60													
Cuckoo.																	
Wt. in g.	1	—	7.56	11.16	16.20	21.24	25.20	28.16	31.68								
	2	7.02	7.20														
	3	6.66	9.00	12.60	19.80	22.68	26.28	28.08	30.60								
Cedarbird.																	
L. wing in mm.	1		12.50	14.00	15.00	19.00	24.00	27.00	29.30	37.00	38.50						
	2		11.00	13.00	15.00	17.00	20.50	23.00	24.80	33.00	38.00						
	3		12.00	13.50	14.00	17.00	20.00	24.00	26.50	33.00	36.50						
	4		10.00	12.00													
Cuckoo.																	
L. wing in mm.	1		15	20	25	31	32	38	43								
	2		17														
	3		17	23	28	31	36	43	45								

Cedarbird No. 4 was probably starved by its more vigorous mates, after the second day. Cuckoo No. 2 fell out of its nest. "0" indicates the egg.

cedar waxwing, and has based his results upon a much larger number of cases. He also considers that the three stages enumerated correspond to stages in the development of muscular coordination, of association and the instinct of fear. During the first period, when the power of motor coordination is weak, according to this observer, "the first crude discriminations and associations are made," and the first signs of instinctive fear noted. In the intermediate period (fourth to seventh day), discrimination improves, and association is perfected, while from the beginning of the last period "there is an abrupt change in all the reactions, the food-reaction ceasing for all the artificial stimuli, excepting occasionally for the visual, and fear begins to develop rapidly through several forms of manifestations."

Kuhlmann recognizes five different manifestations of fear, beginning with "cessation of the food-reaction to stimuli that at first aroused it," and ending with "escape from the nest when approached." Discrimination and the formation of associations between the food and certain stimuli are thought to develop simultaneously, and "all stimuli with which no pleasant associations are already formed are then at the same time instinctively feared." The food-reaction is not only modified by association, but is inhibited by fear, and while the development of association is gradual, the passage of one manifestation of fear to the next in order is often very abrupt. Such animals, he says, "come to fear particular things not so much because of unpleasant associations that are connected with them, as because the taming process has not been completed."

We can not accept the conclusion of the writer quoted above, that "fear for particular things remains in the main instinctive." Fear and association, as we have seen, are without doubt developed, like all else, by a gradual process, however abrupt certain reactions of fear may appear. The normal and usual reactions of daily life seem to go through a sifting process; the usual pass readily through the sieve of experience and are stamped as harmless by association, provided they are really harmless, or at least not disagreeable. Further, there seems to be left a residue of strange or unusual sights, sounds or tactual stimuli, ready to produce the fear manifestation, at a moment's call, when this particular stage in the developing instinct has been reached. The reaction is instinctive, but in no true sense would it seem to be the inherited fear of any particular object or thing. Fear of objects having particular, inherent qualities, which are harmful or unpleasant can come only from experience of their harmful or disagreeable effects.

Habits of Young Kingfishers.—In my work on "The Home Life of Wild Birds," I have described some curious habits which kingfishers show when taken from their underground nest at an early age, especially the habit of sitting still, and of walking backwards. The earlier observations were made over ten years ago, and thinking that some other questions might be involved, such as the rising instinct of fear, experiments were repeated on another family of these birds in the summer of 1908. When dug out of the ground on July 8, the five young in this case were found lying twenty-eight inches below the surface, at the end of a six-foot tunnel; they were in "pin-feathers," and according to my estimate about eight days old. Experiments were made on the ninth, fourteenth, twentieth and twenty-third days, when the young were at an age approximately corresponding to the date, with the following results; fear did not seem to play any part as a disturbing or inhibitory factor in their behavior during the first two days; they would go forward or backward, rather indiscriminately, whatever their position might be with reference to the observer, and whatever the nature of the surface upon which they were placed. On subsequent days, the tendency to walk backward increased, and though fear was rising, they were readily quieted, and when placed in certain positions they would sit quiet for long intervals.

The following notes were made on the behavior of these kingfishers on the last day of observation, July 23; when placed on the pine carpet, all began to make off with fluttering wings, going forward with crests erect and rattles sounding. When recovered and placed in line, they soon quieted, and the backward walking movements began (see Figs. 31 and 32). All showed the same tendency, and one, in which it was especially marked, would retreat four feet before the camera could be focused, and this was repeated for the twentieth time. The same per-

formance was given on level ground, or an incline, and whether facing the observer or away from him, though with variable movements in many

FIG. 31. FIVE KINGFISHERS ABOUT TWENTY-THREE DAYS OLD, removed from underground and placed in line, to illustrate habit of keeping still, and of walking backwards.

FIG. 32. THE SAME KINGFISHERS one minute after the position assumed in Fig. 31 was photographed, showing that Nos. 1 and 5 have each taken a few backward steps, while the pose of Nos. 2 to 4 has not appreciably changed.

cases. When placed in head-to-tail line, and in contact, the line would remain unbroken for a surprising length of time, with hardly the turn of a head, which seems to be due to their habit of sitting still, with

wings often interlocked, during their long imprisonment in a dark, subterranean chamber. When, after a good rest in this position, they were brought to right-about-face, two immediately moved backward a few steps, and came to rest again (Figs. 31 and 32). Move around them in circle, and not a head is turned; make a pass of the hand suddenly towards them, and sometimes there is a slight backward movement, but sometimes there is none; reverse again with heads turned away, and two turn part way around, one of which repeats the movement when repeatedly reversed. If placed on their backs, they will slowly right themselves, though if in the nest-hole they were sometimes contented with this position for a longer time.

I am now convinced that the earlier conclusion was correct, and that the peculiar actions described are due to habit, learned underground, and in relation to getting their food, although I was probably in error in supposing that their instinct of fear was ordinarily delayed until they were ready to leave the tunnel. When such birds are handled daily there can be little doubt that this instinct is liable to be checked. The arched chamber of sand in which these kingfishers lay was 11½ inches in diameter, and 6½ inches high, while the tunnel leading to it had a 3-inch bore. Its temperature stood at 77° F., and was seven degrees cooler than that of the air outside. Since these five birds were each about six inches long it is evident that they were closely packed, and that once in the tunnel, no turning movement would be possible, any more than for the adult which after feeding always backs out of the hole. Now we have earlier noticed a tendency among the older young to crawl down the passageway, and meet the parent at the mouth, but that they are hustled back and presumably fed at the nest, hence the probable association between walking backwards and getting food, and hence the curious habit displayed by these birds when they are taken from the ground.

III. Intelligence in Adult Birds

There seems to be little intelligence displayed by birds in regard to the quantity of food served at the nest. What one of the altricious kinds really does in effect is to "test" the reaction of the throat of its nestling, and to await the response. If this is not forthcoming the food is quickly withdrawn, and another is tried. The most responsive bird gets the food, and there is no distribution on any other basis than this. The same bird may thus be fed twice or even three times in succession, the strongest usually getting the most, and the amount which it can take being reflexly determined by the gullet.

On the other hand, intelligence is certainly shown in the kind of food served, and in the treatment which it often receives. Thus a gull

chick when but a half-hour old gets only small bits of predigested fish, but at the age of three weeks it may be invited to bolt an entire squid. Again a bird like the black-billed cuckoo, which has repeatedly tried to serve a large insect and failed, has been seen to quickly withdraw it, mince it fine with her bill, and then offer it with success.

Birds quickly acquire a habit of going to their nest, by a definite path, through association, and if the branch which holds it is suddenly removed, they try to follow the established course, and will hover at the point in space which the nest formerly occupied, even when their young are in full sight, and these actions may be repeated many times, until the old habit is broken by an actual visit to the new position (compare

FIG. 33. FLICKER FEEDING YOUNG, with nest-hole opened at the back, illustrating the force of old habit. The bird for some time continued to enter the hole by the usual course.

Figs. 33 and 34). The habit of entering the nest from a certain side, of facing the same way while sitting over the eggs, of grasping the same branch when inspecting and cleaning the nest, and of leaving it in a definite manner, are all more or less fixed by habit in a brief course of time. In the same way drinking and bathing places, perches, spots for dusting, sun-bathing and sleeping are resorted to by habit for longer or shorter periods, according to the other conditions which modify behavior.

Do birds discriminate their own eggs and proper young? Very many do not, yet some do, sooner or later. The success of the European

cuckoo and the American cowbird, the young of which are reared by foster-parents of many diverse species, would argue for little power in this direction. Yet, in some cases, the foreign body is promptly removed, when the nest is not summarily deserted through fear.

The freedom with which certain finches and grosbeaks learn to imitate difficult notes, and the fair degree of precision with which some

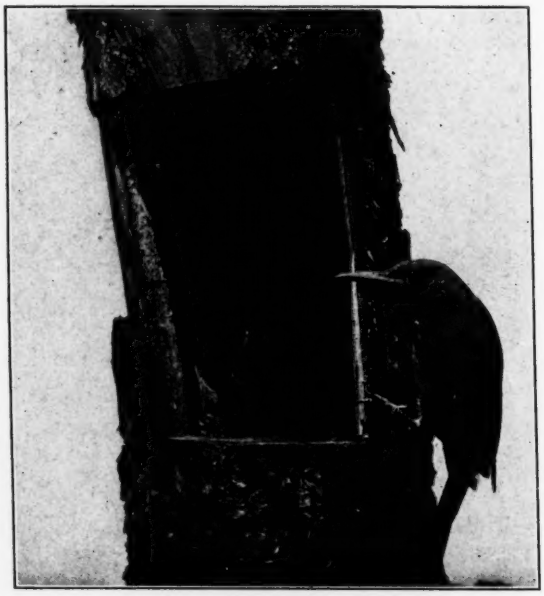

FIG. 34. THE SAME FLICKER as shown in Fig. 33, after new habit of entering opened nest was formed.

of the parrots, crows, jays, jackdaws and magpies reproduce spoken words, or even short sentences, show that they readily discriminate differences in the pitch of sounds, although they do not possess a cochlea of the complexity of structure found in mammals, and it is the cochlea in which this power is supposed to reside in man. It is interesting to note that the magpie, though a star performer in this art, never exhibits it, according to Blackwall, in a state of nature.

The bower birds of Australia show a decided liking for bright and colored objects of various kinds, which they work into their remarkable "runs," bowers, or "play-houses," and the crow, and other members of his tribe, which are commonly regarded as the most intelligent of birds, can seldom be trusted in the presence of any small and shining objects whatsoever, which they will steal, and either carry off and hide, or work into their nests. To mention a trivial case—a tame young crow once entered my room, made off with some objects on the dressing table, and deposited them on a belfry-roof hard by. Again, the hooded

crow in Egypt and India, where from a long and undisturbed intercourse with man, it has come to build its nests in the city streets, and in Cairo even before the foliage of the lebbek trees is out, often gives free rein to this propensity, as was well shown by the experience of an optician in Bombay, who lost a large store of steel spectacle-frames, and later found them in a ruined state, worked into a nest of this familiar bird. The propensity to seize bright objects, and to hide and store food by burying it in the ground, a practise attributed to the European crow, raven, magpie and rook, is undoubtedly instinctive in origin. Their ability to find it again would depend more upon intelligence than in the dog, which has the same tendency, for they are presumably without the guiding power of scent. The Californian woodpecker (*Melanerpes formicivorus*) is noted for the autumnal stores of acorns which it embeds in the bark of trees, but the strong instinctive impulse which shapes its conduct is accentuated by the reported fact that the holes so nicely drilled are occasionally filled up with stones.

That color plays an important part in the lives of birds seems highly improbable, although it is a commonplace fact that the nest in many cases harmonizes perfectly with its surroundings. For several seasons I made a practise of offering colored yarns, such as blue, brown, green and bright red, to various species of birds, for building purposes, and especially to robins and cedar waxwings; as a rule, all colors were taken indiscriminately, with very bizarre nests as a result. When white threads or long streamers of cotton cloth were added, these were usually taken first, and in greater quantity, apparently because they were more conspicuous, and sometimes to the detriment of the builders. Thus, one of the least flycatchers took and dropped so much of the cloth that a white trail was finally laid from field to nest, in the construction of which five times more was used than needed. The quaint structure which resulted was too obvious to escape destruction, and it did not endure many hours.

The docility of birds is well illustrated by the trainer's power over many species, and by the tricks which, through a system of rewards and punishments, they can be made to perform. A classical illustration is furnished by the art of falconry, the popular sport of middle-age Europe, in which the young of the wild peregrine falcon, or of some other hawk, was trained to limit its instinct to kill to a particular kind of game, to follow the falconer afield, to stoop to the quarry, and return to its master's call. After a similar fashion the instincts of the cormorant have been molded to the will of man, and successfully used in taking fish, a practise which I am informed may still be witnessed in certain remote fishing communities in Japan, the trained birds descending from father to son.

Modern experiments in the laboratory, which have been conducted

by students in recent years, by the Hampton Court maze or labyrinth method, upon young chicks, and various wild species, show an ability to learn more or less rapidly, according to the simplicity of the path to be traversed. They always seem to be guided in large measure by sight. Their educability has been further tested by Thorndike and others, by placing food within sight, but enclosed in a wire box, access to which can be reached only by working some simple contrivance, with bill or foot, such as pecking or pulling at a string. The animal is thus induced to do an unusual thing, or to do it in an unusual way, but some species, like the house sparrow, have proved apt to learn, and though success may come first through accident, by the tenth or some later trial, the new act is learned, and unnecessary movements are in time eliminated. The effect of the acts performed, as in the case of exit from the labyrinth, is remembered for days or weeks, according to the strength of the habit, or the ability of the learner. Whether the memory involved in these and similar acts is of a visualized character, involving a memory idea, image or picture, may be doubted, though Edinger among others is not inclined to admit this. We might ask why a bird, with a memory image of the position of her nest, does not always strike a direct path to it, after reaching her tree. Why should she slavishly follow the track stamped in by previous associations, walking along a certain branch, and grasping a certain twig, before landing at the nest-side, a practise very commonly followed? Such behavior certainly can not always be attributed to the inhibitory effect of fear.

All the intelligence which birds may on occasion exhibit seems to give way under the spell of any of the stronger instincts, as when the male canary, as related by Blackwell, plucked the feathers from the necks and backs of its own young in order to line a newly built nest, although other feathers were supplied to it in abundance. They seldom meet emergencies by doing the intelligent act, and, in spite of the anecdotes, probably but seldom come to the effective aid of their companions when in distress. On the other hand, I have more than once seen a mother bird try to pluck a hair or piece of grass from the mouth of a nestling.

It has been asserted that only birds can be frightened from fields by scarecrows, but to most birds any strange object is a "scarecrow," which may in time, and often brief at that, become familiar through association, as shown by the many devices used by farmers to frighten crows from their fields of newly planted corn. The genuine scare *crow* is a subject worthy of further study.

At this point I wish to notice certain anomalous actions of peculiar interest in birds, and to refer particularly to the wood swallows (Artamidæ) of Australia, the hornbills (Bucerotidæ), of the East Indies, and to the honey-guides (Indicatorinæ), of the East Indies and Africa.

The wood swallows are social, gregarious birds of rather small size, characteristic of the tropical forests, where they feed upon insects, and often "hawk" them, like the swallows and swifts. Many have the curious habit of "swarming," or clustering in cold or wet weather in sheltered places or under trees, possibly for the purpose of keeping warm, though this appears to be an assumption; when thus bunched, they crowd one upon another, all heads up, thus forming a great ball-like mass several feet in diameter; if disturbed they go off with startling effects produced by the whirring of many wings, often leaving, it is said, a few dead ones behind, which might have been smothered in the press. All this is suggestive of rheotropism, or the tendency shown by many fish, insects and other invertebrates to orient themselves in response to currents of air or water, and in particular of the clustering tendency shown by the young of many aquatic animals, as well as by many flying insects. Whatever its history may prove to be, no one can doubt that the act is purely instinctive in origin. We are reminded of the swarming habits of chimney swifts, which have been known to enter hollow trees in great numbers for the purpose of roosting and passing the night, especially after their arrival in spring and before their fall departure.

Hornbills are large birds of peculiar structure, and wide distribution in the old world, being noted for their great serrated bills, which in many of the species are surmounted by a remarkable casque or helmet. But it is in the cyclical instincts of their reproductive period that we find the most extraordinary departure from the common type. Before she is ready to lay her eggs, the female hornbill enters some suitable cavity, in a dead tree or branch, and with or without the assistance of the male, proceeds to wall herself in, closing the opening with mud or excreta, or with both, with the exception of a hole large enough to admit the bill, and the food which is passed in by the male. While thus confined, the female lays the eggs, incubates them, and through the cooperation of her mate their naked and helpless young are reared until ready for flight; then the prison-house is suddenly burst open, the enfeebled mother and the young are liberated, and the happy family united in the bright world outside. Further, at intervals during this period the male casts off and regurgitates an inner layer of the gizzard, which with all the contained food comes up like a dumpling, that is to say, a package or thin-walled sac, three inches long by two inches in diameter, and upon this generous food-supply the female is able to subsist for some little time.

The practise of closing the opening to the nest is to be regarded as a modification of the nest-building instinct, and while its history has no doubt been lost in the remote past, it may be compared with a not wholly dissimilar practise of the European nuthatch, which also nests

in natural cavities, and when the entrance is large regularly blocks the passage with mud until it will barely admit her body. The hornbills have possibly lost the cleaning instinct, if they were ever possessed of it, and the singularity of their present activities must be attributed to instinct alone.

The little honey-guides are related to the barbets, and hoopoes, rather than to cuckoos, although like many of the latter they are thought to regularly steal the nests of other birds, and never rear their proper young. But aside from this diversion, they are said to conduct the passing traveler to bees' nests, to call his attention to the important business in hand by hisses and shrill cries, and to even fly in his face "as if enraged at not being followed." That such efforts are not wholly altruistic may be gathered from the fact that they will eat the bees, grubs and honey alike. According to the accounts, the honey-guides are the "pointers" among birds, for when the woodsman is encountered, they flutter up to him and point the way to a nest, and if followed, go on and on, but halt when hot on the trail. They will also point to empty nests, or even to a domestic hive, but more significant than this, they will follow a dog, or lead the confiding traveler to a leopard, cat or snake, showing clearly that, whatever the origin of this practise, whether concerned with the instinct to sound the alarm at a common enemy, and to follow it and keep it in view, or not, we are dealing with an instinct; and probably one of very pure type.

We will close this account by giving one or two reputed instances of bird-intelligence which stand out in a marked degree from others of their kind, on account both of the acts themselves and the credibility of the witnesses. Thus Montagu, whose excellence as an observer is abundantly proved in his "Ornithological Dictionary of British Birds," states that he once saw two crows (*Corvus corone*), by the seashore "busy in removing small fish beyond the flux of the flowing tide, and depositing them just above high-water mark, under the broken rocks, after having satisfied the calls of hunger." It seems to me that too much has been made of this instance, since it may with equal justice be interpreted as an illustration of the instinct to hide, the circumstance of the tide being fortuitous, for it does not follow that these birds knew that the tide would surely advance and sweep away their prize. The incident, however, is interesting in relation to another, told of the hooded crow (*Corvus splendens*), by the worthy Blackwall, who saw these birds "on the eastern coast of Ireland, after many unavailing efforts to break with their beaks some of the mussels on which they were feeding, fly with them to a great height in the air, and, by letting them fall on the stony beach, fracture their shells, and thus get possession of the contents." Perhaps it would not be easy, says Blackwall, "to select a more striking example of intelligence among the feathered

tribes than this, where, on one expedient proving unsuccessful, after a sufficient trial had been made of it, another was immediately resorted to."

A similar habit has been attributed to the gull, but with how much truth I am unable to say; the exact history of its origin, in either case, would be of much interest. It would seem to have arisen either from accident or from ideas, for the question of imitation can here be ruled out, so far as the initial performance in a given individual is concerned. The very rarity of the habit, attested by the fact that it has been so seldom reported, seems to clearly point to an accidental origin, and to the conclusion that it does not rise above the level of associative memory.

THE PALEONTOLOGIC RECORD. III

ANATOMY AND PHYSIOLOGY IN INVERTEBRATE EXTINCT ORGANISMS

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THE inquiry into the position of anatomy and physiology in invertebrate paleontology seems very appropriate at present, since paleontology is steadily becoming more closely affiliated to zoology, and the sphere of zoology is at present dominated by comparative anatomy and physiology.

Since, however, invertebrate paleontology has only the hard parts, mostly outer shells, at its disposal, the view still prevailing among zoologists that little is to be expected from it in regard to the solution of the problems of anatomy and physiology of the lower animals seems natural. Nevertheless, the results already attained prove that if paleontologists do not approach their material with a geological knowledge only, as has been done in the past altogether too often, most gratifying results may be obtained, at least in some classes, for it must be conceded that the connection of the hard parts with the fleshy parts is very unlike in different classes; it is very intimate in some, as the crinoids and brachiopods, and again more indifferent, as in the gastropods.

But it is not claiming too much for invertebrate paleontology if we say that where the hard parts are of great structural importance, paleontology has earlier taken cognizance of this fact and consequently gone ahead of zoology. As an instance I may cite Zittel's investigations of the skeleton of the hexactinellid sponges which have taught the fundamental importance of the form of the spicules and the structure of the skeleton in that class and whose results have been readily adopted by zoologists. In classes which, as the brachiopods and crinoids, are to-day mere shadows of their former greatness, paleontology has its greatest chance, and it would fail in its task if it would there not become the instructor of the affiliated science; and it is gratifying to see that this fact is finding recognition, as, for instance, in Ray Lankester's "Treatise of Zoology," where the chapter on the crinoids has been entrusted to Bather, a paleontologist and one of the best authorities on crinoids.

It is apparent that in such classes as those just mentioned, of which only the last ends of the branches are still alive, the origin and nature

of many structures can not be elucidated, even by the embryology and comparative anatomy of the recent forms, but only by paleontology. Such a structure is, for instance, the mystifying stem of the crinoids which, by a study of the primitive ancestors of the crinoids among the cystids, is readily recognized as a dorsal evagination of the body. Likewise, to cite another example, the siphuncle of the recent *Nautilus,* which is obscure as a wholly rudimentary organ, is in such primitive Paleozoic cephalopods as *Nanno* and *Piloceras,* still seen in its original form and thereby recognized in its nature.

Since that which has already been accomplished in fossil anatomy is proof that there are still larger fields to be ploughed and harvested, it is proper to inquire into the best methods of this work before us.

We first need more extensive and more intensive or more detailed purely descriptive anatomical researches of the invertebrate fossils. There are many species that, when investigated in their smallest detail, are bound to give important results. I may cite here, as examples of such accomplishments, Hudson's minute study of the strange *Blastoidocrinus* of our Chazy rocks with its 90,000 ossicles, or that of the *Eurypterus fischeri* by Holm. Of this archaic fossil marine arachnoid, a relative of the scorpion and of the king crab, it can be fairly said that, as far as its chitinous integument is concerned, it is as well known as any recent species. We know, through Holm, its gills, its complex genital appendages of both sexes, and even its fine hairs and bristles. Dr. Clarke and myself have lately continued these investigations in the American eurypterids, and there observed the structure of the compound eyes, the pore system of the segments, the genital apertures, the mode of moulting, the arrangement of some of the principal muscles and other anatomical facts of interest.

It can be said that this field of detailed descriptive anatomy has been merely touched thus far, as far as our fossil invertebrates are concerned, and altogether too much neglected. This is not only true as to the gross anatomy, but still more so as to the microscopic structure. It must be conceded that owing to the secondary changes in the rocks, this latter line of investigation meets with great obstacles not fully appreciated by the zoologist, and that it is only in its infantile stage in some classes. But the results obtained by the microscopic research of the Paleozoic bryozoans in this country may be considered as a striking example of what persistency and enthusiasm may still accomplish. In microscopic anatomy of the fossils the training of a geologist is as much required as that of a zoologist and the history of these investigations shows that a zoologist without geologic training may be badly misled by the deceptive states of preservation of the fossils.

The main object of anatomical research is to result in comparative anatomy and to determine what parts are fundamental or primary and

what have undergone modifications due to functional changes. It is obvious that here invertebrate paleontology is in a position to answer a host of questions that could not be successfully approached by comparative anatomy of recent forms, by the direct observation of successive changes. Its methods of investigation have already been applied with wonderful success to large parts of our Paleozoic crinoids, brachiopods, bryozoans and cephalopods. And I do not doubt that the time has come when the preliminary stage of mere description of fossils is passed, and a monographic treatment of each class that would fully enter into the comparative anatomy of all structures preserved, could be profitably undertaken.

It is only by this work that paleontology can hope to make those contributions to philosophical anatomy in revealing the causes of the different structures which it is especially fitted and called upon to furnish by its ability to study the gradual development of the structures. Wherever a class of fossils has been thus thoroughly treated, it has given a fruitful crop of new hypotheses and principles, as is instanced by Hyatt's investigation of the fossil cephalopods. Most classes, and especially the corals, echinoids and trilobites, await such treatment by competent investigators.

Since physiology is that branch of biology that treats of the laws of phenomena of living organisms, it might seem hopeless to expect any information from the fossil world. This is apparently the more true in regard to the invertebrates, since a special physiology exists thus far only for men and the higher invertebrates and the recent invertebrates are largely a virgin field. For this reason also, only the most general foundations of comparative physiology have been laid, and an invertebrate fossil physiology would get as yet but little support from that side. Moreover, the main source of exact information in recent physiology is the experimental method, and this is wholly inapplicable to the fossil world.

And yet it seems to us that the empiric method upon which physiology has so long flourished promises also rich fruit in paleontology. I can do no more now than briefly mention the problems that most readily suggest themselves here. Invertebrate paleontology will be especially competent to furnish contributions to the mechanics of physiology by throwing light on the development of the means and modes of locomotion. In connection with this problem invertebrate paleontology also shows most clearly the deep-reaching influence of secondary fixation on the structure of the organism, as in the case of the strange *Richthofenia* among the brachiopods and the Rudistæ among the lamellibranchs. It can not fail that the progress in recent invertebrate physiology will stimulate inquiry into the physiology of the fossils; and further that, as invertebrate fossil anatomy progresses, the data for such inquiry will also come forth.

Another problem closely connected with that of the mode of locomotion is that of the origin of the organs of sense, and also upon this, as far as the organs of seeing at least are concerned, the fossil invertebrates are able to throw some light, as in the trilobites and eurypterids.

Another line of inquiry is that of the mode of nutrition as recognizable by the appendages, and its influence upon the general structure. Under this heading such interesting minor problems as that of the origin of parasitism arise and may be solved, as indicated by a recent publication as to the time of beginning, causes and gradual changes of parasitism, to its very complex present conditions.

Probably also the physiology of respiration will in time receive important additions as far as the echinoderms, crustaceans, scorpions and eurypterids are concerned.

The widest scope, however, will have those problems that are connected with the reactions of the organisms to their physical and chemical surroundings. The invertebrate paleontologist meets forever, in sight of the ever-changing faunules, the question, what exterior influences caused these changes? Often they can be directly recognized, as in the dwarfed faunules of the Devonic pyritiferous Tully limestone or of the bituminous Marcellus and Genesee shales or the eurypterid faunas of the Salina lagoons. The systematic investigation of these reactions through the series of formations is an inviting task.

A special problem of singular interest connected with the reaction of the organisms to the chemical surroundings is that of the composition of the shell of the invertebrates. There is good evidence for the view that the shells were at first chitinous and that but gradually they became calcareous or siliceous. This important question again is intimately connected with that of the original composition of the ocean, and this line of inquiry again leads us to the highly fascinating paleophysiological problem, lately so happily dealt with by Professor Lane, as to the geological evidence on the original composition and origin of the vital liquid, the original body temperature and the physiological origin of the hard parts of the invertebrates in general.

CONTRIBUTIONS TO MORPHOLOGY FROM PALEONTOLOGY

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OUR knowledge of the morphology both of the animal and plant kingdoms has been largely extended by the work of the paleontologist. Mention needs only to be made of the many species, genera and families, even orders and classes, established solely for fossil forms

to show how much we owe to paleontology. There is not a single subkingdom but has been immensely enriched from this source.

Some of the fossil species possess morphological characters so closely allied, on the one hand to earlier, and on the other to later, forms as to indicate that they occupy a position in the line of descent, and phylogenetic series have been established frequently on this basis. As examples we have the well-known developmental series of the horse and the camel. Other illustrations may be found in the *Paludinas* of the Slavonian Pliocene and in the *Planorbis* types of Steinheim.

Still other fossil forms combine in the same species several morphological features which later become segregated and characterize different types. Such "synthetic types" serve to show the common origin of the forms in question if not their actual ancestors and have greatly enlarged our knowledge of the morphology of the several groups involved. These early forms are, for the most part, highly generalized, while their descendents are variously specialized. Take, for example, the mammalian *Condylartha,* small, generalized Ungulata with an astragalus shaped almost as in the Carnivora; or the reptilian *Anomodontia* with intermediate skeletal characters between the highest labyrinthodonts and the lowest mammals; or again, the early Paleozoic cystoids with generalized characters in their calyx plates which appear in more specialized forms in later crinoids and blastoids. An almost indefinite number of such illustrations might be cited.

Still other fossil forms present morphological characters so different from other fossil or living species that the genetic relationships may not be determined accurately. Some of these are possible of reference to already defined orders, while others present so many diverse morphological characters as to require the establishment of new divisions for their reception.

A survey of the known fossil and living forms shows that not only have old species constantly become extinct during the progress of geological time, but new species have been as frequently appearing. This is equally true of genera, families, orders or even classes. Some forms have appeared and disappeared, as the case may be, suddenly; others slowly. The great group of the Ammonites, for example, disappeared suddenly at the close of the Cretaceous after showing many degenerate characters, while the Trilobites gradually declined during late Paleozoic time before their final extinction. One of the most striking features in the developmental history of plants and animals is found in the great number of fossil types which have left no descendants.

Both the animal and plant kingdoms furnish a wealth of material with which to demonstrate the aid which paleontology has rendered to morphology.

The contributions of invertebrate paleontology are numerous and striking:

The *Protozoa* afford in the Carboniferous Fusulinidæ and in the Tertiary Nummulinidæ forms with very different morphological characters from those living to-day, while the numerous extinct species of the Lituolidæ and Textularidæ in the Cretaceous and of the Miliolidæ and Globigerinidæ in the Tertiary have greatly widened our knowledge of the entire subkingdom.

The *Cœlenterata* in the Paleozoic Tabulata and Graptoloidea show types so different from living forms that the systematist has never been able to satisfactorily assign them to a position within the limits of the phylum. Many external and internal characters appear that are quite unknown in later forms. On the other hand, the paleontological subclass of the Tetracoralla long imperfectly understood is now regarded with a fuller knowledge of the morphology as affording the probable ancestors of the later Hexacoralla.

The *Echinodermata* have furnished two classes, the Cystoidea and the Blastoidea, unknown after the Paleozoic, whose morphology aids very materially in an interpretation of later and more highly differentiated forms among the Pelmatozoa. Thus the cystoids, which have been regarded as the ancestral type from which the crinoids have sprung, afford forms like the Camarocystites, in which the arms are similar to those of the crinoids although the calyx plates are irregularly arranged and thus cystoidean in character. Both the Asterozoa and Echinozoa are represented in the fossil state by many species that greatly widen our knowledge of the morphology of this group. Take for example, the Echinocystites, regarded as belonging to the Palechinodea which has a valvular pyramid of calcareous anal plates so highly characteristic of the cystoids.

The *Molluscoidea,* to which phylum belong the Bryozoa and Brachiopoda, would be but imperfectly understood from a morphological standpoint but for the vast number of fossil forms. The Brachiopoda have been estimated to have less than 150 living species, while probably more than 6,000 fossil species have been described. Of the 31 families only 7 have living representatives. We are dependent, therefore, largely on the fossil forms for our knowledge of the morphology of this class.

The *Mollusca* with their varied forms, although so well represented to-day, have furnished in the fossil state one of the most interesting and important orders in the animal kingdom, the Ammonoidea with its 5,000 and more species ranging from the Devonian to the Cretaceous. Even the allied Nautiloidea, although containing living forms, attained its chief development in the Paleozoic, and it is from these ancient forms that we obtain our chief knowledge of the morphology of this group with their early straight and irregularly coiled types.

The *Arthropoda* afford in the Paleozoic the important groups of the trilobites and euripterids, forms that have aided greatly in the inter-

pretation of the entire phylum. The trilobites from their morphological features have been generally regarded as entomostracous crustaceans with relationships on the one hand to the Phyllopoda and on the other to the Merostomata, while the coalescing of the caudal segments suggests also a relationship to the Isopoda.

Vertebrate paleontology has also furnished much to morphology.

The *Fishes* would be but imperfectly known in their wonderful variety but for the fossil types. The problematical group Agnatha found only in the Silurian and Devonian affords no certain evidence of a lower jaw or paired limbs, and in some of the genera of the Ostracoderma mimic in a curious way the contemporaneous euripterids, which has led some to erroneously ally them with the Merostomata. The dermal armor of most of these forms is also a striking morphological feature.

Woodward divides the fishes proper into Elasmobranchii, Holocephali, Dipnoi and Teleostomi, and considers that the common ancestors of all were Elasmobranchs. Numerous fossil forms among the Elasmobranchs and Dipnoids as well as the Crossopterygians which have been thought by many to bridge the gap between the Telelostomi and Dipnoi have added largely to our knowledge of the phylum.

The *Batrachians* which consist to-day largely of diminutive forms were represented in the later Paleozoic and early Mesozoic by the Stegocephalia which contain the giant labyrinthodonts with their highly complex infolding of the walls of the teeth.

The *Reptilians* which began their existence toward the close of the Paleozoic became so numerous and diversified during the Mesozoic that this division of geological time has been referred to as the age of reptiles. Several orders of Saurians containing many giant types flourished during this time, but became practically extinct before the close of the period. With the adaptation of some for walking on their hind legs, of others for swimming, and still others for flight we have developed a great variety of morphological features that would never have been suspected from a study of living forms.

The *Birds* which are recognized as possessing certain dinosaurian relationships and were doubtless derived from one of the reptilian orders are unknown prior to the Jurassic. The Mesozoic forms are generalized, the tail at first not being atrophied and the pelvis imperfectly developed as in later forms. The vertebræ also had not acquired their saddle-shaped articulation while teeth were present in the jaws of the adults. Such forms certainly add greatly to our knowledge of the morphology of this class.

The *Mammals* which began in the early Mesozoic were represented throughout the Cenozoic time by highly diversified forms, many of which have left no descendants. The gradual evolution of the mam-

malian skeleton has brought about many morphological modifications from those shown in the Batrachia and Reptilia. We find the skull loses the prefrontal and postfrontal bones, the mandible is simplified, the limb bones show a development of terminal epiphyses with ossification to the center of the vertebræ and the bones of the pelvic arch are ossified. From the beginning of the Tertiary time a marvelous variety of morphological characters appears, and without the fossil types we should have but an inadequate conception of this great phylum.

The contributions of paleobotany to morphology are in some respects quite as striking as those of paleozoology.

The fossil *Thallophytes* have not furnished any very striking variations from their present morphological features, while the *Bryophytes* are scarcely represented as fossils except in very recent deposits.

The remaining phyla, the *Pteridospermatophytes,* the *Pteridophytes* and the *Spermatophytes* have their oldest known beginnings as far back as the Devonian and their study has enormously widened the bounds of plant morphology.

The *Pteridospermatophytes,* which are confined to the Paleozoic, are in habit and vegetative morphology ferns—in methods of reproduction and in the morphology of their reproductive organs typical seed plants. They alter our whole conception of ferns and seed plants and in their significance are comparable to archetypal vertebrata, the acquisition of the seed habit in plants and the vertebral column in animals probably marking the culmination of the transfer of vital activity from aquatic to terrestrial conditions.

In the *Pteridophytes* the extinct Paleozoic class, the Sphenophyllales, is significant, since the morphology of the distinct lycopod and *Equisetum* lines seems to merge in this group. The lycopod type, itself represented in the existing flora by six or seven genera of herbaceous plants, monotonously uniform in their morphology, is found in the Paleozoic to constitute one of the chief units in the arborescent flora with numerous species of complex organization, whose stem, foliar and reproductive morphology was quite unknown to botanists (*Lepidodendron, Sigillaria,* etc.). The *Equisetum* type furnishes a like case. With few existing species of minor importance and uniform morphology we find in the Paleozoic a host of forms, many of them arborescent and of varied and complex structure (*Calamites, Archæocalamites,* etc.). Similar examples could be drawn from the fossil representatives of the true ferns.

In the *Spermatophytes* another wholly extinct class, the Cordaitales, embraces a curiously organized group of conifers extending back to the oldest horizons from which land plants are found, and continuing to the close of the Paleozoic as one of the most abundant as well as the highest type of pre-Mesozoic plant. In the older Mesozoic we find two

groups of plants which have made similar great contributions to morphology. The Cycadales or cycad-like plants, which to-day are an inconspicuous group, were one of the dominant Mesozoic types, and any understanding of the modern forms rests entirely upon a study of their immensely abundant Mesozoic ancestors. The other group, the Ginkgoales, represented in the existing flora by a single species, the ginkgo, is found in the Mesozoic to have been represented by many genera and species of great diversity.

The dominant plants of to-day, the conifers on the one hand, and the angiosperms on the other, have each afforded many extinct genera, the former with more fossil than recent species, and only understandable in the light of their fossil ancestors. Vegetable morphology based only upon existing plants abundantly demonstrated its sterility before the relative recent study of fossil plants placed it upon an altogether new basis.

RELATION OF EMBRYOLOGY AND VERTEBRATE PALEONTOLOGY

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THE problem of recapitulation among vertebrates gives by no means as accurate results as among invertebrate forms, for while a single adult shell, if perfectly preserved, will often display the entire life history or ontogeny of the individual, a bone, or even a complete skeleton, is rarely retrospective and if at all only in some minor detail. The vertebratist, therefore, in his study of ontogeny, for comparison with racial history must needs follow either the entire growth of one animal, a thing manifestly impossible when the embryonic stages are considered, or study a long series of individuals in various stages of development, the securing of which in the great majority of cases is largely the result of a number of happy accidents. When one comes to weigh the evidence offered by the actual embryos of fossil vertebrates he will find a very great dearth of material, for fossil embryos—that is, the stages in the life history before birth or hatching—are extremely rare.

Recent embryology, on the other hand, is more productive of results and the earlier stages of certain organs often suggest those of equivalent development in animals of the past. In his interpretation of a given structure, however, one has to bear in mind whether it may not have been modified to suit some modern need in the life history of the individual, and thus no longer give us a true image of bygone structure. These cœnogenetic organs are not historic, but as Wilder says, "have to do with such immediate environmental problems as nutrition or protection." Again, if the organ has approximately the same form

and character in the ancestral type at the same stage of its development, it represents an actual repetition of past history and is therefore palingenetic. Sometimes it is not quite clear, however, under which caption the embryonic structure comes, and its interpretation must be attempted with caution.

Osborn in his lectures to his students speaks of the three-fold evidence for evolution which stands firmly like a tripod, the legs of which are comparative anatomy, embryology and paleontology; and the evidence of each should correspond, provided the interpretation be correct. Of these, however, embryology is manifestly the weakest member, while paleontology is a tower of strength!

The reptiles are so rare as embryos and withal so ancient a group that their ontogeny throws but little light upon paleontology. Among the fossil forms a number of specimens of *Ichthyosaurus* have been found with young contained within the body of the adult. Many of these are in the normal position for fœti-in-utero; others are displaced, with the head directed forward. These latter Branca thinks may be young that have been eaten. There is also, at times, a very great difference in the size of the contained young. Aside from a slight difference in proportions, especially that of head to trunk, and a less degree of hardness of the embryonic bones, as indicated by their being crushed over the parent's ribs, the young teach us nothing as to ancestral structure as they are in every way perfect ichthyosaurs. They do prove, however, when the evidence of viviparity which they offer is taken in connection with the supreme degree of aquatic adaptation indicated, that the ichthyosaurs were high sea-forms, never coming ashore even for egg-laying.

That certain of the dinosaurs were also viviparous may be proved by an embryo contained in the unique specimen of *Compsognathus longipes* from the Jurassic of Bavaria. So far as I am aware this embryo gives no other evidence of ontogenetic value.

The turtles have been made the subject of exhaustive study by Hay and from the embryological point of view by Clark under L. Agassiz. Anatomically they are the most remarkable of reptiles, having undergone during their career an extreme modification in many directions while retaining a number of very primitive characters. The most remarkable feature is the development of the shield or carapace, which contains what are generally considered as the homologues of the ribs of other vertebrates, but, strangely enough, here lying outside of the shoulder girdle, a feature wherein the turtles are utterly unique. The embryology, which is well known, ought to throw some light upon the origin of this important feature. In their earlier stages of development the Chelonia resemble the Lacertilia, the chief pecularity being caused by the development of this carapace which appears in the form

of two longitudinal folds extending above the line of insertion of the fore- and hind-limbs which have already made their appearance. Hence the carapace grows outward and over the limb-girdles which come to lie within the rib-like osseous supports. This ontogeny shows us clearly how the ancestral carapace may have been formed. Paleontology has not as yet confirmed this, but doubtless will do so.

Among birds one of the most interesting features is the occurrence of vestigial tooth papillæ in the jaws of certain embryo parrots and owls —reminiscent of Mesozoic days when birds were toothed in their adult state.

Mammalian evidence is very striking in many details and much of it has recently been summarized by Hubrecht, who makes much of the character of the placentation and derives from it and other features some remarkable conclusions. Hubrecht abandons the idea of the derivation of the mammalia from the reptilian-insectivorean stem, but on the contrary derives them from amphibia-like animals of the Carboniferous. The character of the placentation, moreover, places man, the Anthropomorphæ and the hedgehog among the most archaic of living mammalian types, an idea also borne out by comparative anatomy and one which paleontology may some day confirm.

The most primitive mammals, the Prototheria, are still very suggestive of their old reptilian ancestry in many ways, especially in the method of producing the young inclosed within an eggshell. The position of the Marsupials is surely low in the scale of mammalian life, although they show in many respects remarkable specializations. Wilder compares them with the Prototheria in that they also bring forth their young at a very early state of development, though not protected by an eggshell. The period during which they are permanently attached to the nipples within the pouch is actually post-embryonic and properly larval. Vestiges of placentation have been found in at least one marsupial, a fact which gives color to the belief that in this respect they may be degenerate rather than primitive. Broom has shown that the modern marsupial shoulder girdle passes through a prototherian stage implying a relationship which is strongly supported in other ways.

The fœtal Sirenia and Cetacea, so far as known, show no greater development of hind-limbs than do the post-natal individuals. They do show a marked neck construction and the head bent at right angles with the trunk in a normal quadrupedal posture. The head of the fœtal manatee is very suggestive of the modern ungulate. Ryder has tried to prove the homology of the caudal flukes in the Sirenia and Cetacea with the integument of the hind feet, drawing his evidence largely from comparison with the seals. In the embryo the flukes appear as lateral expansions near the end of the tail, giving it a lance-like form when viewed from above. These flaps by transverse expan-

sion develop into the powerful swimming flukes of the adult. They may be compared with lateral flanges on the tail of the sea otter *Enhydris,* but in the latter the flaps are elongate, while in the Cetacea they are short and situated toward the end of the tail. Nevertheless, the homology of the two types of flange structures appears true, the posterior position and concentration in the whale being a mechanical adaptation which has become accelerated in its appearance so as to be embryonic. The presence of hair on the body of the fœtal whale and of distinct calcareous tooth germs in both upper and lower jaws of the unborn young of whalebone whales are both reminiscent.

The horses, our knowledge of which is so complete owing to the pioneer work of Marsh and later of Osborn, show some interesting points of comparison between fœtus and ancestor. The skulls of prenatal modern horses resemble those of *Mesohippus* or even of *Eohippus* in the proportions of face and cranium, the short-crowned grinding teeth, lesser angle between basi-cranial and basi-facial axes and the fact that the orbit is incompletely ringed with bone. The feet of the unborn foal are also somewhat reminiscent of old-time conditions.

One of the most difficult points to be reconciled in the acceptance of the Cope-Osborn theory of the origin of molar cusps was the apparent non-agreement of cusp ontogeny with the interpreted phylogeny which this theory upheld. The difficulty has been met in two ways: by the supposition that cœnogenesis has entered into the embryogeny, or that the paleontological record as shown by the trituberculists is open to a different interpretation. The present great exponent of the idea claims that the matter is still *sub judice* and thus the problem stands.

In conclusion, the paleontological student of the higher vertebrates can hope to find in embryology a host of valuable suggestions, much verification of his work and sundry apparent inconsistencies which must in some way be reconciled. He should ever bear in mind the influence of nature and nurture, the latter often giving rise to perplexing conflicts between the two records. He will on the whole have in embryology a fair mirror of the past wherein, even though the image be somewhat distorted and the more remote reflections dimmed by time, he can view the striking features of the long procession of the ages.

OSERVATIONS ON THE EARTHQUAKE OF MAY 26, 1909

By Professor J. A. UDDEN
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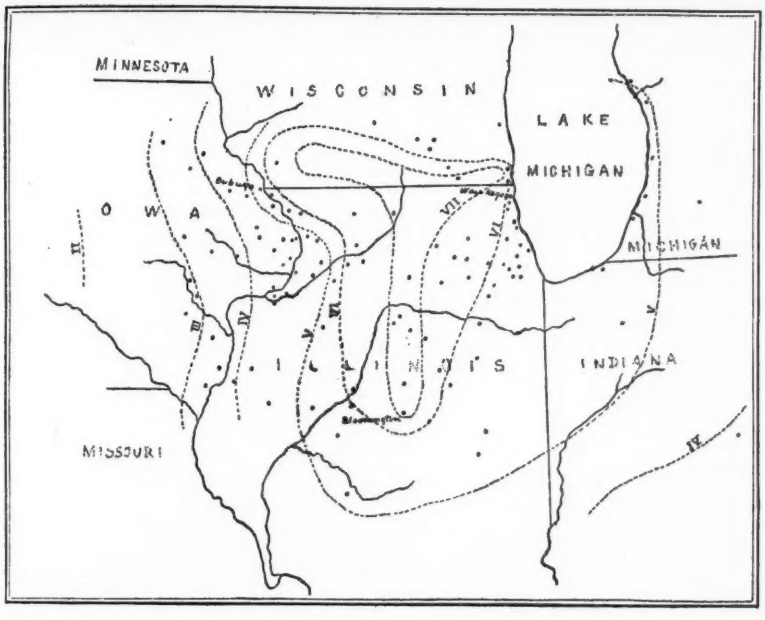
EARTHQUAKES are infrequent in the upper part of the Mississippi Valley, and observations on earthquake phenomena in this part of the world have a peculiar interest, not only on account of the special bearing they may have on seismological questions, but also on account of the light they throw on the psychology of an observant public which is unacquainted with seismic phenomena. The writer wishes to present some observations on the earthquake which occurred in this region on the twenty-sixth of May, 1909. They are based on notices which appeared in the public press, and which were secured from fifty daily and weekly newspapers immediately after the earthquake. The collected reports contained in all some three hundred observations on incidents which occurred during a few moments shortly before nine o'clock in the morning, when the earth waves rapidly traversed the states of Illinois, Iowa, Wisconsin, Michigan, Indiana, Missouri and Minnesota.

The reports collected indicate that the mesoseismal area of this earthquake lay in northern and north central Illinois, and reached slightly beyond the south boundary of Wisconsin. It appears also that there were no less than three epicentral tracts, one near Dubuque in Iowa; one near Waukegan, and another near Bloomington, in Illinois. At all of these places the shock was strong enough to slightly damage a few buildings. From this unusually large triangular mesoseismal area the earthquake waves spread in all directions, sensibly as far north as to Rochester in Minnesota and to Muskegon in Michigan, east as far as to Muncie in Indiana, westward to DesMoines in Iowa, and southward to Hannibal in Missouri, affecting an area of some five hundred thousand square miles.

In the central region, where the earthquakes are most complex, one report states that a distinct raise, or upward motion, was first felt, and that this was followed by a trembling. In other cases, houses and floors are said to have "heaved." In Beloit the houses are said to have been "jostled out of plumb." Violent shaking and rocking is also reported. Farther out from the central area there is a more frequent use by the reporters of such terms as "shaking," "rocking," "swaying" and "jarring," while toward the outer margin of the disturbed area houses are more often said to have been gently rocked and shaken, or to have "trembled" or "quivered," indicating the more gentle and



ISOSEISMALS OF THE EARTHQUAKES IN THE Upper Mississippi Valley, May 26, 1910.

more regularly undulating nature of the free and gradually vanishing oscillations of the earth.

The greater number of earthquakes are now known to be due to slipping of enormous blocks of the earth, along fissures or joints of great depths, and thus forming the dislocations known to geologists as faults. In the case of the earthquakes with two maxima of disturbance, the slipping occurs first at one point in such a fissure, and then at another. There can be no doubt that this Illinois earthquake was of the nature of such a compound slip, although the exact position of the fault can not be correctly located from the data at hand. In most descriptions of the shock *no* mention is made as to whether there was one maximum or more. Such particulars were naturally overlooked. The people of the upper Mississippi Valley are not trained in making observations on earthquakes. Nevertheless, nine observers make mention of more than one commotion. One account from each of eight localities states that two distinct shocks were felt. These places are Bushnell, Canton, Champaign, Chicago, Geneva and Sterling in Illinois, and Davenport and Dubuque in Iowa. In the latter place the first disturbance lasted about ten seconds, after which there was a short pause and then again a shock of short duration. But the reports from Chicago, Springfield and Champaign, which places lie on the other side of the mesoseismal area, all agree in stating that the first shock was of brief duration, and that the second lasted several seconds. One observer is reported as having noted three distinct shocks, and this was Professor W. H. Hobbs, at the time on a visit in Madison, Wis. He is

one of America's prominent seismologists, and his testimony may be regarded as specially competent. The three shocks he noted may have been three separate impulses coming from the three epicenters previously mentioned, at Dubuque, Waukegan and Bloomington, started by slippings closely following each other in each of these places.

This inference is in a measure strengthened by some observations made on the duration of the earthquake. There are in all fifty-eight such observations, showing a range of estimates from one second to three minutes. Thirty-eight of these estimates vary from one to eight seconds and average four seconds. In six places the disturbance is reported to have lasted ten seconds; in five places, fifteen seconds, and in one, twenty. An average of these twelve estimates is about thirteen seconds. In two places the shock is reported as lasting a half minute; in three places, one minute; in one, a minute and a half, while in Dixon and Joliet the disturbance continued for three minutes. No great accuracy can be claimed for these estimates, but it will be observed that they fall into three groups, one with an average of four seconds, one with an average of about thirteen seconds and another with an average of about sixty seconds. We may suppose that the shortest average represents places where only one of the three shocks was sensible, while the two larger averages represent places where two or where all three shocks were strong enough to be felt. All places where the disturbance lasted more than a minute are somewhat centrally located, and may hence very well have been exposed to the effects of all the three shocks, each of which increased the total length of the period of the disturbance.

No less than sixty-six observations are reported on the time at which the earthquake was felt. These are of interest chiefly in showing how great is the difference in accuracy of time measurements required for general purposes, and for the purpose of seismic investigations. They also illustrate our general preference for round numbers. The reports range from eight o'clock in the morning to twenty minutes after nine. More than half of them give the numbers thirty, thirty-five, forty and forty-five minutes after eight. Discarding these figures, which are multiples of five, twenty-two observations range from thirty-seven to forty-one minutes after eight. The time recorded by the seismometer in the office of the United States Weather Bureau in Peoria, no doubt more reliable, was thirty-eight minutes after eight. The time marked by another government seismometer in Washington was forty-one and a half minutes after eight. If the velocity of the earthquake wave in traveling from Peoria to Washington, be calculated from these last two figures, we find that it approached three and three tenths miles per minute. For the purpose of determining the velocity of earthquake waves the data furnished by the press reports are of course entirely inadequate.

The location of the epicentral tracts and of the mesoseismal area is,

however, clearly indicated by reports from which the intensity of the motion may be estimated, and from which isoseismal lines may be constructed.

The data contained in the press reports can be readily compared with the Rossi-Forel scale of intensities. The greatest intensity shown is in the falling of chimneys and in the cracking of walls, which barely approximates eight in the scale. It is not practical to separate these localities of greatest intensity from a more extended region where the earthquake had an intensity more nearly comparable with seven in the scale. Within this area furniture was overthrown, plaster fell from ceilings and from walls, and hanging pictures and other suspended ornaments were jerked loose from their fastenings. Outside of this most severely disturbed mesoseismal area there is a belt from ten to a hundred miles wide where the intensity approximates the next lower point in the scale. Here lighting fixtures, chandeliers and bookcases are reported to have swayed, dishes were broken, chairs rocked or were moved or overthrown, houses were rocked, chimneys cracked and clocks were stopped. Beyond this again is a zone where the evidence of the earthquake consisted in the more subdued motions described as shaking of houses and of furniture, rattling of dishes, bottles and tinware and swinging of suspended objects. This zone has a width of from fifty to a hundred and fifty miles and marks the location of the fifth isoseismal. Continuing the diminuendo, the earthquake next announced its rapid passage by the rattle of windows, the jarring and quivering of houses, and by gentle shaking and trembling of furniture. This is the fourth intensity, and characterizes a zone that merges imperceptibly into the next, where few people noticed the disturbance, and where it appeared as a merely perceptible jar, or a slight undulation, most frequently noted only in the upper stories of high buildings. In this gentle form it disappeared to human senses at a distance, in all directions, of some four hundred miles from the central region. How much farther did it speed, unseen, unheard and unfelt? You will remember that it left a record on the seismometer in Washington. This city is nearly four hundred miles beyond the zone where the waves ceased to be perceptible to the unaided human senses. From this record we may infer that in the brief span of two or three minutes the earthquake waves spread over a circular area about sixteen hundred miles in diameter.

A classified review of the little things that happened in the upper Mississippi Valley, when a block of the earth slipped in the northern part of Illinois may perhaps be of interest. The phenomena reported, affected at least five of the human senses, the senses of general well-being, of touch, of equilibrium, of hearing and of sight.

A rheumatic woman in the zone where the disturbance was very feeble "felt the vibrations keenly and told others of the earthquake,

before it was generally known." Another woman, in poor health, ascribed the peculiar feeling that she experienced to an attack of heart disease, and sank frightened in bed. Experiences of this kind have been noted in other earthquakes and appear to be due to a morbidly excitable condition of the ill-defined and unspecialized sense of general well being. Some people perceived the earthquake chiefly through the sense of touch, as when a man, seated in a chair and resting his legs on a railing, "felt his legs shake," or as when a chief of police, also seated, felt that his chair shook. In several other cases the earthquake was likewise merely "felt." No doubt the sense of touch entered as an important element in a far greater number of instances when mention is made that something shook, trembled, quivered or rocked, or when there was a jar or a tremor. The sense of equilibrium or of poise was evidently involved in the case of a man who felt "dizzy," and in the case of people who "wabbled on the streets," in cases where occupants of houses noted a "heaving," "rocking" or a "swaying" motion, and when people "were thrown down," or "nearly tumbled over," or "found difficulty in keeping on their feet."

The reports mention only five instances of sounds accompanying the earthquake. Such sounds are general in the mesoseismal area in all severe earthquakes in all parts of the world, except in Japan, and one noted seismologist believes that their absence in that country is due to a racial inability among the Japanese to hear sounds of very low pitch. The general absence of sounds in the Illinois earthquake is readily accounted for by its comparative weakness. It was faintly audible only in three epicentral tracts. Some parties claim to have heard a distinct rumbling before the shock in Dubuque. In Waukegan one man described the quake as a rush of wind, and said that he had heard it. This swishing noise is one of the many known characteristic forms of earthquake sounds. In Springfield, Ill., a faint rumbling was heard, and a janitor in one of the school buildings in Peoria made a similar observation. One man heard a sound like the "bumping of a locked door." This is another variation of earthquake noises, which, when more powerful, resemble volleys of musketry and artillery, and which, like the other noises, originate under the ground. Many observations involve sounds which are, as it were, proxies of the quake, induced by secondary events, such as the rattling of windows and dishes, the crash of falling brick and the like. The student of earthquakes depends, as we have seen, on such noises for much of his information on the progress of the earth waves in the peripheral region of the disturbed area.

The sense that gives us the most reliable information on earthquakes, as on most other physical phenomena, is the sense of sight. Visible earth waves are, however, rarely seen except in severe disturbances. It is uncertain whether they appeared anywhere in this case,

but there is a suggestion that they were noted in Waukegan, where the sidewalks were seen to "heave." A high bridge near Dubuque, Ia., and some old buildings in Plattsville, in Wisconsin, were seen to "sway." But motions of such structures, as well as motions of smaller objects, indoors and outdoors, are merely the effects of the earth's motion and not a part of the earthquake itself. It is unnecessary to enumerate them here.

Another classification of the earthquake phenomena takes into consideration the different objects giving evidence of the seismic motion and the terms used by the observers in stating how these objects were affected. It presents simultaneously in this case, a study in journalistic diction and in mechanics. Forty reports relate the varied behavior of buildings and houses. These are said to have shaken (17), rocked (7), trembled (4), swayed (3), cracked (3), to have been jarred (2), to have quivered (2) and to have creaked and heaved, respectively in as many cases as indicated by the inserted figures. Observations on dishes, bottles and tinware are next in number. These mostly rattled (15), or were broken (8). Some were dashed to the floor (6), others merely fell (3), some were shaken (3), were moved (2), or they rocked, trembled, wabbled and were disturbed. A crockery store in Dubuque sustained a damage of some eight hundred dollars. The words used in describing the motions of furniture present a turbulence of performances of almost kaleidoscopic variety. Chairs, tables, beds, bookcases, even sedate stoves shook (8), were moved (6), were overturned (3), swayed (2), quivered (2), trembled (2), broke, were upset, tipped over, threatened to tumble over, rattled, rolled back and forth, rocked, heaved and "had the glass shattered." Windows and doors rattled (12), and shook (3), and one door was "sprung so it would not close." Hanging pictures and mirrors engaged in a variety of diversions. Some swung (3), some were shaken from the walls (3) and some were thrown from their fastenings. One mirror "trembled on the wall," and another "fell from the wall." One is said to have "jumped around" and one was "demolished." Light fixtures and lamps swayed (4), heaved, shook, were shaken from their rests, were overturned, fell and broke, or were knocked down, and one gas flame was "shaken out." Chimneys fell (4), toppled over (2), were razed, shaken down and cracked. Water and milk in tumblers and pans were spilled (5) and tippled. At Sabula a wave was thrown up against the bank of the Mississippi River. Bric-a-brac was shaken off, tipped off and thrown down from mantles (6) or simply fell to the floor. Four clocks were stopped. Heavy machinery rattled or "shook in good shape." An elevator swayed and some linotype machines swayed violently. Telephone wires were seen to sway, a telephone receiver was knocked off its hook, and a telephone instrument was "put out of commission." Other public utilities suffered serious damage, several thou-

sand dollars being needed for the repair of disjointed gas pipes in Chicago and its suburbs. Goods were thrown from the shelves in some stores. In the watch works in Elgin some delicate instruments were thrown out of gear, and in a printer's office in Dubuque some type, locked in a form, was pied. A kitten was thrown across a room.

There was one class of accidents, some serious and others comical, which could not have been foreseen as the results of an earthquake. These involve some trigger-like arrangements. Falling stoves and disjointed stove flues caused several fires in Aurora and Chicago. In Waukegan the shock disarranged the bins in a feed store, and some of the grain was let out through a crack between the boards. The leg of a piano was loosened and fell in a school in Oak Park. The whole instrument was in this way upset and tumbled down on the floor, and the accompanying crash and noise naturally frightened the children.

Many reports describe the mental state and the behavior of people on experiencing the unusual sensation of the earthquake. In the epicentral tracts some were terrified, many left, or fled or rushed from their homes, or from buildings where they were working. There were several small panics among laborers and among employees in factories. People were alarmed and excited and ran on the streets. Some schools were dismissed for the day and instruction was interrupted in two university classes. From farther out in the disturbed region some papers state that the people in the upper stories of some high buildings were frightened, and from still further out reports mention that people were surprised or merely that they perceived the physical sensation, evidently unattended by any emotion.

The tendency of the human mind to make inferences and draw conclusions is pointedly illustrated by many of the reports. In cases where the earthquake was not recognized, the disturbance noted was nevertheless invariably ascribed to some cause, more or less remote, but suggested through the bias of previous experience. Many people thought the jar they felt was due to an explosion or a blast in some quarry, and others thought it was due to the moving of some heavy object in the building they occupied. A janitor in a school building thought that a man engaged to repair the flag pole had fallen on the roof. A grocer who had piled up some sacks of flour in the second story, went up to see if these had fallen down. People living near car lines and railroads referred the commotion to passing cars or trains. Residents in the cities were reminded of the passing of heavy vehicles. Two unsophisticated children jumped out of a bed that shook, ran crying to their mother and reported that the bed was falling to pieces. A young lady stenographer in Chicago, more versed in the ways of the world, felt her chair rocking and promptly rebuked a supposed offender at her back with the command: "You stop that."

Projected forward instead of backward, reasoning results in the

vision of things impending. Here also the bias of earlier experience and of training plays an important rôle. Remembering the recent disasters in Europe, Italian laborers in Chicago quit work to fall on their knees and pray. Recalling a prophecy of the coming of the end of the world three days later, some Zionists are reported to have concluded that the earthquake was the beginning of the fulfillment of the prophecy. Some persons who had left their houses, refused for hours to enter them again, fearing a repetition of the earthquake. A prisoner in a jail is said to have speculated on his chances of getting away, in case the walls of the jail would fall, and some people in Chicago feared the coming of a "tidal" wave from Lake Michigan.

It is well known that afferent impulses, especially if they are powerful, have the effect of inhibiting or interfering with central psychic activities. Such inhibition was probably responsible for the forgetfulness of a reporter who sent in his account of the earthquake in a neighboring city to a newspaper in Clinton, but forgot to affix his signature. It explains also the action of a woman in a hospital, who was walking on crutches and who ran out without them, to escape from the building. With the inhibition of man's reason, his instincts take its place, and it would seem that many of our instinctive actions are not much different from those of the brute. They are exemplified in the panics that took place in a few factories and schools. When people rushed from buildings and started to run on the streets, they acted on instinctive impulses. These actions must have been prompted by a nervous mechanism quite like the mechanism that started several runaway horses in places where the earthquake was sufficiently severe to appear alarming. The launching of sensational rumors during a general excitement is traceable to a related instinct, only more refined and exclusively human. The reflex was started on this occasion by a fire in a kitchen in Aurora, and the reaction announced that "Aurora is burning up."

One phenomenon in this connection is almost embarrassing to mention, in view of the present growing sentiment in favor of women's rights and woman suffrage. It appears from the effects of the recent earthquake on the American people, that human reason is more readily inhibited in the gentler sex and in children, than in men. The statement may be worded in another, and perhaps a better way, by saying that human instincts are relatively stronger in woman than in man. This statement will hardly pass as anything new. This distinction is implied in the wording of one report, which states that "men were excited, women and children frightened." It is stated that in Dubuque a panic was narrowly averted in a shop where women worked. In an office building in the same city it happened that the women rushed in a panic to the stairs, and that men met them and quieted them. In a home for young women the jar is said to have "scared the

occupants out." Several panics occurred in schools. One man relates that his wife and sister "rushed to him." Nurses were alarmed in a hospital. Telephone girls left the switch-boards in Chicago, and "were scared" in Clinton. A particular mention is made of a seamstress who was alarmed, and of another woman who sank frightened on a bed. But in no case is a man specially mentioned as having been afraid. In places where men were scared, fright was general, and there was then no cause for such special mention. The evidence of this difference can hardly be charged to an unconscious discrimination by the reporters in favor of the stronger sex. It must be regarded as a noteworthy incident in this earthquake that its intensity was near that limit, which is strong enough to scare women but not men. This limit must approximate seven in the Rossi-Forel scale, and the unsentimental seismologist may hence add another criterion for correctly locating the seventh isoseismal.

One general observation which has a practical bearing should perhaps not be left unmentioned. It is that the earthquake was more strongly felt in the upper stories of high buildings than on the ground floors. In Dubuque "the upper part of the high buildings swayed." A reporter in Burlington says that the shock was "felt most in the upper stories of tall buildings." "The floors shook in the upper stories of large buildings" in Clinton, and in Davenport "the tremors were mostly noted in high office buildings." In Chicago the shock was not felt on the ground floors, but mostly "only in the higher stories." The top floors are especially mentioned as having shaken in some of the university buildings in Evanstown and in a college building at Cedar Rapids. In the architecture produced by the demands of industry and business in this part of the world, the eventuality of a severe earthquake has not entered as an element of consideration. The experience of a half century shows that this neglect is probably justified. Nevertheless, it is appalling to contemplate how different the story of this recent jar would appear if the intensity of the disturbance had been just a little greater than it was. From our past experience we may safely infer that the valley of the upper Mississippi is in a region where earthquakes are not frequent. Are we also justified in believing that when such disturbances do occur, they will not be severe? The violence of the New Madrid earthquakes a hundred years ago makes the answer to this question uncertain. Time alone will tell.

THE METHODS AND USES OF A RESEARCH MUSEUM

By JOSEPH GRINNELL

DIRECTOR OF THE MUSEUM OF VERTEBRATE ZOOLOGY OF THE UNIVERSITY OF CALIFORNIA

THE average public museum contains natural history specimens of two categories—those which are displayed within glass cases constantly open to the light, so as to be continually in the view of visitors; and of those which are stored away in various appropriate containers, ordinarily protected from the light, and which are not open for the inspection of the general public, though they may be freely handled and examined by the special student in the field to which they pertain. The former category of specimens constitutes what is usually referred to as the museum proper, or exhibition museum; while the latter forms what may be termed the research museum.

The functions of an exhibition museum have been discussed at length, and its claim to recognition as a valuable factor in public education as well as amusement has been too well established to require further proof. It should be remembered, however, that much of the material on display may at the same time be of direct value in research; for it consists in part of such objects as skeletons which are not affected injuriously by light and which may be encased with a view to easy access by the osteologist who wishes to examine them minutely.

It is in the research department of the museum that I believe lies a great value, even though the sight-seeing visitor may know nothing of its existence. The maintenance of a research department on a large scale is certainly justifiable, as I purpose to show, by the importance of the results to be obtained through it from the standpoint of pure science. In an institution, like the Museum of Vertebrate Zoology, which is an integral part of a large university, it may even be warrantable to emphasize the importance of research over exhibition. For the presence of the research museum serves as a stimulus to the university student and as a source of material and information usable in the work of other departments in the university.

In discussing at length the functions of a research museum, in order to have something concrete to use in illustration, I will refer constantly to the institution with which I am connected. Here, although it has been little more than two years since its inauguration, enough of methods and policies have been formulated to furnish data for the basis of this paper.

The functions of our research department, in other words the

energies of our curators and the expenditure of our money allowances, are directed along the following lines:

Our most obvious activity, though not necessarily the most important one, lies in the accumulation of the preservable remains of animals of the vertebrate classes with the exception of those below the Batrachia. I am sure that no one will disagree with me in the claim that the results of our work will be of far greater moment in thus narrowing down the object of our work to a portion of the animal kingdom than if we were to spread it thinly over a greater range of subjects.

The field of our work is the region immediately about us. In other words, it is much less effective to attempt to secure a representation of the animals of the world than to exploit the fauna of a limited area. The Pacific coast is practically inexhaustible, is naturally of easiest access and should be of greatest interest to this institution.

Our collections consist of the skins and skulls of mammals, each individual collected being ordinarily represented by its skin, together, of course, with all dermal structures attached, and the entire skull, cleaned and preserved separately. The entire skeletons of a much smaller proportion of the specimens secured are also preserved; and of the smaller forms the entire animal, a few of each species, is preserved in alcohol for anatomical purposes.

In the class of birds the ordinary study skin is the chief portion preserved. However, the endeavor is made to secure complete skeletons representative of each family at least; and also portions of skeletons of a greater number, consisting of skulls and sterna chiefly. As with the mammals, alcoholic preparations are saved, especially of young birds. The expense and mechanical inconvenience of collecting and storing alcoholics impose a practical limit upon the quantity of material to be cared for in this way.

Reptiles and batrachians are preserved entire as alcoholics. Skeletons should also be prepared and saved, but the difficulty of properly obtaining them has proved so great that as yet we have but few. At any rate, with the entire animal preserved in alcohol it is possible for the special student at any time to take out the skeleton of the reptile or batrachian that it is desired to study.

The museum's policy is, and should be everywhere, liberal as regards the loaning of material to non-resident as well as near-by specialists. Material of any sort is loaned freely to any responsible person any where for the purpose of aiding in his investigations, or as basis of any special study. The value of a museum's hoard of specimens and facts increases in direct ratio to the extent to which they are used. No museum is a success as long as it remains a cold-storage warehouse, closed to ready access by the general student whether he be remotely situated or located within easy reach.

The museum curator only a few years since was satisfied to gather and arrange his research collections with very little reference to their source or to the conditions under which they were obtained. In fact it is surprising to find how little information is on record in regard to collections contained in certain eastern institutions as accessioned previous to about 1885. The modern method, and the one adopted and being carried out more and more in detail by our California museum, is to make the record of each individual acquired, whether it comes in from an outside donor or whether, as is the most usual case, it is secured by the trained museum collector, as complete a history as practicable.

The field collector is supplied with a separate-leaf note-book. He writes his records on the day of observation with carbon ink, on one side of the paper only. The floral surroundings are recorded, especially with respect to their bearing on the animal secured. The behavior of the animal is described and everything else which is thought by the collector to be of use in the study of the species is put on record at the time the observations are made in the field. The camera is as important a part of his outfit as the trap or gun. These field notes and photographs are filed so as to be as readily accessible to the student in the museum as are the specimens themselves.

Furthermore, a rather elaborate system of card cataloguing is maintained in the museum. Three sets of cards, namely, accession, department and reference, which are kept up as a part of the regular work of the curators, enable the enquirer to determine quickly what material is on hand, in what form it is, when and where obtained, and, by following up the cross references to the field note-books, the conditions under which each animal was obtained.

As a matter of routine, each specimen as it is obtained in the field is at once tagged, the label being inscribed in India ink with the exact place of capture, date, collector and field number. The original field number is the same as that under which the animal is at the same time recorded in the field notes. Its original tag is never detached from the specimen, no matter what disposition is made of the latter in arranging the collections in the museum; and so, reversely, the student may quickly trace back again from any particular specimen its history, by referring to the card catalogue and field note-book. In addition to the original collector's number there is added on each label a separate department number by which it is referred to in the museum records and any published articles specifically mentioning it.

It will be observed, then, that our efforts are not merely to accumulate as great a mass of animal remains as possible. On the contrary, we are expending even more time than would be required for the collection of the specimens alone, in rendering what we do obtain as per-

manently valuable as we know how, to the ecologist as well as the systematist. It is quite probable that the facts of distribution, life history, and economic status may finally prove to be of more far-reaching value, than whatever information is obtainable exclusively from the specimens themselves.

At this point I wish to emphasize what I believe will ultimately prove to be the greatest value of our museum. This value will not, however, be realized until the lapse of many years, possibly a century, assuming that our material is safely preserved. And this is that the student of the future will have access to the original record of faunal conditions in California and the west wherever we now work. He will know the proportional constituency of our faunæ by species, the relative numbers of each species and the extent of the ranges of species as they exist to-day.

Perhaps the most impressive fact brought home to the student of geographical distribution, as he carries on his studies, is the profound change that is constantly going on in the faunal make-up of our country. Right now are probably beginning changes to be wrought in the next few years vastly more conspicuous than those that have occurred in ten times that length of time preceding. The effects of deforestation, of tree-planting on the prairies, of the irrigation and cultivation of the deserts, all mean the rapid shifting of faunal boundaries, the extension of ranges of some animals, restriction in the ranges of others, and, with no doubt whatever, the complete extermination of many others, as in a few cases already on record.

If we now had the accurate record of faunal conditions as they were in the Atlantic states a century ago, how much might we not be able to adduce from a study of the changes which have taken place. Now is the opportunity to make such records in our western region. Comparative studies of conditions in the same area at different successive times is bound to bring important generalizations in the field of evolution. It will be seen here how valuable also will prove the collections preserved at corresponding intervals. Changes in conditions will doubtless bring about changes in the habits and physical characters of the animals enduring them.

Another grave danger from the standpoint of the student of natural speciation lies in the introduction of exotic animals. This evil is growing rapidly in the effort to restock regions with more hardy or prolific game animals. If successful from the sportsman's basis, either of two things will happen: the original, native species will become extinct by competitive replacement, or, where the relationships are close, crossing will take place so that the original species will be spoiled through hybridization. There are already instances of both in different sections of the United States. It is highly desirable that a good repre-

sentation of specimens of the pure, native stock be properly preserved in our museums, for future comparison.

I wish here to register an objection to the prevalent idea that experimental methods upon the higher animals under artificially imposed conditions may be expected to lead invariably to the satisfactory solution of evolutionary problems. I have in mind some experiments recently made upon birds. Certain species were kept captive in enclosures in which a relatively high atmospheric humidity was maintained. The experimenter found that within the life of an individual, in fact within a few months, successive molts resulted in the plumages of some of the birds becoming darker. Feathers which were normally marked lightly with black became solid black. The increase of pigment throughout the plumage brought about a conspicuous change in the appearances of the birds, as great a difference as one finds between two near-related species under natural conditions, the one occupying an area of arid climate, the other a region of humidity.

The conclusion from these few experiments, quite generally, but, I feel confident, too hastily, drawn, has been that there may be a "direct influence" of the atmospheric humidity sufficient to bring about the color characters of the different species as we find them under the varying natural conditions; in other words, that it is not a matter of gradual adaptive acquisition subject to inheritance. It is even being maintained widely among biologists that natural selection may have very little to do with the characters of animals as we find them in nature.

I believe that the above experiments, among others carried on in the same way, will, alone, lead to inductions largely inapplicable to animals in the wild. My chief objection is that wild animals brought into confinement at once begin to show irregularities in various structural respects. This is shown sufficiently by studies upon the skeletons of animals dying in zoological parks, a very large proportion of which are abnormally modified in various particulars. This diseased condition undoubtedly begins just as soon as the animal is taken out of its natural surroundings. For the cessation of any one set of muscular activities is bound to bring about immediate changes in quantitative metabolism in the system. Change in food supply directly affects the entire organism, and unusual invasion by parasites ensues with concomitant irregular growths. How then can we expect to get a knowledge of the processes of species formation under natural conditions from the extraordinary physical development or behavior of such animals?

I would urge that it is only through the close and long-continued study of animals in the wild state, that is, under perfectly natural conditions, that we can hope to gather conclusive evidence as to the

causes and methods of evolution. Our research museum has assumed the rôle of recorder of faunal conditions as they are in this age. I reiterate, for emphasis, that I believe its greatest ultimate value will not, therefore, be fully realized until a later period.

But to return to our immediate activities and their justification: The mass of information already at hand brings us face to face with numerous problems of distribution and variation. As our field work is carried on, we learn more and more in detail of the extent of the range of each species of animal, and we are able to recognize more clearly the correlated factors. We are able with more accuracy to define the characters of the local races or subspecies. The study of these "small species" I believe is leading to a better understanding of the relationships of animals and the causes of evolution than if we ignored the slight varieties and contented ourselves with dealing systematically only with the species differentiated so far as to be distinguishable at a glance.

Systematists, either as members of our museum staff or students from elsewhere, who make use of our material, are putting on published record the more important facts of distribution and variation as they come to light. All of this activity leads to the more thorough knowledge of animals necessary for any sort of wider generalization. Our institution is a repository of facts; and no matter what may be said to the contrary by those who undervalue the efforts of the hoarder of facts, it must always be the mass of carefully ascertained facts upon which the valid generalization rests. I have lately learned from no less than three zoologists of prominence that the published scientific paper which does not include some induction or generalization is not worth while. The result, it seems to me, of such a sentiment as this, which is being promulgated among the younger students, is to encourage premature conclusions. The object, in the view of the young research student, becomes the discovery of generalizations, and he is liable to be content with a wholly insufficient basis of facts. We can not expect satisfactory inductions from scanty data any sooner than from inaccurate data. At the same time I do realize that the ultimate value of the facts lies in their service as indicators of general truths. The amassing of detailed facts in any field of science is certainly a commendable pursuit; and if generalizations of wide application are early indicated, so much the better. Our research museum is a repository of facts.

There is a more widely-appreciated function of our institution which is already asserting itself as an important one in the research museum's activities, especially in its connection with a state university: People want to know whether or not a reptile is poisonous; whether or not a bird is beneficial or injurious; whether or not a

"wild animal" is to be feared. People instinctively want to know the names of things. There is the mere curiosity, perfectly laudable, which brings such questions as these to the museum in greater and greater number. It is as a popular source of information that no small part of the curator's time is occupied.

The economic value of birds and mammals to the agricultural interests of the state is one of practical importance. In our field work we obtain a great amount of information applicable along this line; and, further, our staff keeps posted as to the results of the important work carried on by our national government to ascertain the beneficial or injurious effects of wild animals. Either from knowledge acquired directly by ourselves, or from that published elsewhere, we are often able to give the information asked for. The museum is thus constituted a popular bureau of information as regards the higher vertebrate animals of the region with which we are familiar.

The functions of a research museum may be summarized as follows: Collecting and preserving animals of certain groups from a limited region; recording in permanent form all obtainable information in regard to their distribution, variation, economic status and habits; serving as a bureau of popular information as regards the animals of the region worked in; the description and analysis of ecologic and faunal conditions as they are to-day; the publication of the immediately important data obtained, calling attention to whatever generalizations these facts may point towards.

THE EFFECTS OF SMOKING ON COLLEGE STUDENTS

By Dr. GEORGE L. MEYLAN
COLUMBIA UNIVERSITY

THE question of the effects of tobacco upon the smoker has received much attention from moralists, educators, physicians and scientists. The literature on the subject is voluminous. Numerous investigators have experimented upon animals, mainly to determine the effects produced by nicotine. The results of these experiments show that nicotine when injected in animals acts as a strong poison, causing disturbances of the nervous, circulatory and respiratory functions. The problem of determining the effects of smoking upon human beings presents far greater difficulties than the effects of nicotine injections on animals. There is very little agreement in the conclusions reached by the many physiologists and physicians, who have investigated this problem.

Professor Lombard, of the University of Michigan, has shown that in from five to ten minutes after beginning to smoke an ordinary cigar muscular power began to diminish, and in an hour when the cigar was burned, it had fallen to about 25 per cent. of its initial value. The total work of the time of depression compared with a similar normal period was as 24.2 is to 44.8.

According to Dr. Woodhead, of Cambridge University:

> Cigarette smoking in the case of boys, partly paralyzes the nerve cells at the base of the brain and this interferes with the breathing and heart action. The end organs of the motor nerves lose their excitability, next the trunks of the nerves and then the spinal cord. In those accustomed to smoking, it has a soothing effect upon the nervous system, but often acts as a nervous stimulant to mental work, as in reading. In those cases the effect is not due to nicotine itself but to the stimulus of the smoke on the sensory nerves of the mouth, which reflexly stimulate the vaso-motor system and dilate the vessels of the brain. There appears to be less irritation of the brain structure and motor nerves than of the sensory nerves, but the power of fine coordination is decidedly lost.

Dr. Clouston, the eminent English physician, writes on tobacco as follows:

> The use of tobacco has become the rule rather than the exception among the grown men of Europe and America and of some parts of Asia. If its use is restricted to full-grown men, if only good tobacco is used, not of too great strength, and if it is not used to excess, then there are no scientific proofs that it has any injurious effects, if there is no idiosyncrasy against it. Speaking generally, it exercises a soothing influence when the nervous system is in any

way irritable. It tends to calm and continuous thinking, and in many men promotes the digestion of food. To those good results there are, however, exceptions. It sometimes sets up a very strong desire for its excessive use; this often passing into a morbid craving which leads to excess and hurt. Used in such excessive quantity tobacco acts injuriously on the heart, weakens digestion, and causes congestion of the throat as well as hindering mental action. In many people its use tends towards a desire for alcohol as well. I have repeatedly seen persons of a nervous temperament where the two excesses in tobacco and alcohol were linked together. Tobacco, properly used may, in some cases, undoubtedly be made a mental hygienic.

Dr. Pereria says:

I am not acquainted with any well-ascertained ill effects resulting from the habitual practise of smoking.

Dr. Richardson writes of tobacco in the London *Lancet:*

It is innocent as compared with alcohol; it is in no sense worse than tea.

In the Fourth Annual Report of the Henry Phipps Institute, 1908, Dr. Lawrence F. Flick reports that of 443 male patients treated for pulmonary tuberculosis, 72.68 per cent. used tobacco. The result of the treatment was favorable in 38.28 per cent. of the patients who used tobacco, as against 47.42 among non-users. Unfavorable results occurred in 61.7 per cent. of the users of tobacco, and in only 52.62 per cent. of the non-users. Dr. Flick concludes:

Here again, as with alcoholism, we have merely evidence as to the influence of tobacco on the development and mortality of tuberculosis and not upon implantation. . . . The statistics here given, if they have any meaning at all, would seem to indicate that the use of tobacco by males may be one of the explanations why tuberculosis is at present as much more prevalent among males than among females. Tobacco undoubtedly depresses the heart and interferes to some extent with vigorous circulation. It is generally conceded that anything which depresses the circulation interferes with nutrition.

Under the title "The Effects of Nicotine," Dr. Jay W. Seaver published an article in the *Arena,* for February, 1897, in which he gives some statistics of the differences in the physical measurements of smokers and non-smokers among Yale College students. Unfortunately, Dr. Seaver does not give any figures of the actual measurements or the number of cases that he observed. He says:

A tabulation of the records of the students who entered Yale in nine years, when all of the young men were examined and measured, shows that the smokers averaged fifteen months older than the non-smokers, but that their size, except in weight, which was one and four-tenths kilograms more, was inferior in height to the extent of seven millimeters (about ¼ inch), and in lung capacity to the extent of eighty cubic centimeters.

In explanation of the difference in age between the smokers and the non-smokers, Dr. Seaver says:

The difference in age in the two groups points to an age limit to parental restraint, and raises the inquiry as to what might supplement this influence.

In regard to the influence of smoking on the increase of physical measurements of college students, Dr. Seaver says:

The effect of nicotine on the growth is very measurable, and the following figures are presented as a fairly satisfactory demonstration of the extent of the interference with growth that may be expected in boys from sixteen to twenty-five years of age, when they are believed to have reached full maturity. For purposes of comparison the men composing a class in Yale have been divided into three groups. The first is made up of those who do not use tobacco in any form; the second consists of those who have used tobacco regularly for at least a year of the college course; the third group includes the irregular users. A compilation of the anthropometric data on this basis shows that during the period of undergraduate life, which is essentially three and a half years, the first group grows in weight 10.4 per cent. more than the second, and 6.6 per than the second, and 11 per cent. more than the third; in girth of chest the first group grows 26.7 per cent. more than the second, and 22 per cent. more than the third; in capacity of lungs the first group gains 77 per cent. more than the second, and 49.5 per cent. more than the third.

These figures have been widely quoted, and generally considered as affording positive proof that college students who do not use tobacco make far greater progress in physical development than is the case with smokers. Without actual figures of increment in measurements, these percentages signify little or nothing. For instance, the difference of 24 per cent. in stature increment reported might mean that the smokers increased 17 millimeters and the non-smokers 21 millimeters, but no one would attach any significance to a difference of 4 millimeters in stature measurement.

A recent study by E. L. Clarke, published in the *Clark College Record* for July, 1909, shows that 46.3 per cent. of 201 students smoke. The smokers exceed the non-smokers a little in strength and lung-capacity, and 26 per cent. of the smokers won athletic insignia against 16 per cent. of the non-smokers. But in the matter of scholarship, 68.5 per cent. of the non-smokers won honors as against only 18.3 per cent. of the smokers. Mr. Clarke concludes:

1. As a rule the non-smoker is mentally superior to both the occasional and the habitual smoker.

2. As a rule the non-smoker is equal, and probably slightly superior, physically, to all members of the smoking classes except the athletes. It may well be queried as to whether the smoking athlete does not make his gain at too high a mental cost to make it pay. No one would contend for a moment that smoking is the sole cause of these differences. There are numerous other factors that are inseparably linked with it.

The question may be approached from the physiologic, the moral or the economic view-point. In this article, the chief aim will be to determine if smoking exerts any influence upon the physical and mental characteristics of college students; the moral question involved will be considered only incidentally; no attempt will be made to present the economic view-point. The writer, with the cooperation of his assistant, Mr. Hyman Cohen, A.M., made a detailed study of 223 college students from two classes, including all for whom records could be obtained.

PERCENTAGE OF SMOKERS

115 smokers or	52 per cent.
108 non-smokers or	48 per cent.

AGE WHEN SMOKERS ACQUIRED THE HABIT

Age	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
Number	1	0	0	2	0	2	0	11	11	18	30	23	16	0	1

AVERAGE MEASUREMENTS OF 145 COLLEGE STUDENTS AT THE BEGINNING OF FRESHMAN YEAR AND END OF SOPHOMORE YEAR

	Age	Weight	Height	Lung Capacity	Total Strength
68 Smokers, 1st.	18.8	59.1	171.8	4.02	582
68 Smokers, 2d.	20.1	62.5	173.0	4.10	685
Difference.	1.5	3.4	1.2	.08	103
77 Non-smokers, 1st.	18.0	59.0	170 4	4.08	570
77 Non-smokers, 2d.	19.6	61.6	172.5	4.28	671
Difference.	1.6	2.6	1.1	.20	101

The smaller number of observations in the physical measurements is due to two causes: first, the physical examinations are optional for students entering with advanced standing in physical education; these students usually take only the first examination; second, a number of students in this group had not yet taken their second physical examination when this study was made. The selection is therefore purely accidental.

It appears from these tables that there is no appreciable difference between the measurements of the smokers and of the non-smokers except in the matter of age. The slight advantage in the average measurements of the smokers at the first examination is undoubtedly due to the fact that they are 8 months older. The slightly larger gain made by the smokers in weight, height, and total strength during the first two years in college is really too small to have any significance.

SCHOLARSHIP RECORDS OF THE 223 STUDENTS

	Average Marks at Entrance	Marks during first 2 years	Failures during first 2 years
223 students	90 per cent.	66 per cent.	7 per cent.
115 smokers	89 per cent.	62 per cent.	10 per cent.
108 non-smokers ...	91 per cent.	69 per cent.	4 per cent.

The differences in scholarship standing between the smokers and non-smokers are distinctly in favor of the non-smokers.

If the only difference between these two groups of students is that the members of one group use tobacco and the members of the other group abstain from it, then it would appear that there is a direct relation between smoking tobacco and scholarship. A further study of these two groups brings out differences between smokers and non-smokers in athletic and social activities.

STUDENTS WHO WON A PLACE ON ONE OR MORE VARSITY ATHLETIC TEAMS

Of 223 students	84 or 37.6 per cent.
Of 115 smokers	47 or 41 per cent.
Of 108 non-smokers	37 or 34 per cent.

This table shows that 41 per cent. of smokers and only 34 per cent. of non-smokers achieved success in varsity athletics.

Of varsity athletes 56 per cent. are smokers as compared with 52 per cent. of all students.

The following table illustrates the same point in another way, giving a percentage of 57.3 for the number of smokers on the various varsity athletic teams during one college year.

PERCENTAGE OF SMOKERS ON VARSITY ATHLETIC TEAMS DURING THE SEASON OF 1908–09

	No. of Men	No. of Smokers	Per Cent.
Baseball	13	11	84
Soccer	11	7	63
Swimming	14	11	74
Crew	12	4	33
Hockey	7	4	57
Lacrosse	12	6	50
Basketball	6	1	17
Wrestling	7	4	57
Gymnastic	11	5	45
Fencing	3	2	67
Total	96	55	Average, 57.3

Participation in the social activities of college life is best measured by membership in college fraternities. The following table shows the relation between smoking and membership in fraternities:

STUDENTS WHO BELONG TO COLLEGE FRATERNITIES

Of 223 students	66 or 29.4 per cent.
Of 115 smokers	49 or 42.6 per cent.
Of 108 non-smokers	17 or 15.7 per cent.

This table shows a very close relation between smoking and membership in college fraternities.

SCHOLARSHIP OF STUDENTS WHO BELONG TO COLLEGE FRATERNITIES

	Average Marks at Entrance	Average Marks during first 2 years	Average failures during first 2 years
223 students	90.0	66.0	7.0
66 fraternity members	85.4	59.1	12.8
49 fraternity members smokers	84.0	56.6	14.4
17 fraternity members non-smokers	89.4	66.5	8.1

In order to show more clearly the facts brought out in the previous tables, the following comparisons are made:

Of 223 students	115 or 52.0 per cent.	are smokers
Of 96 athletes	55 or 57.3 per cent.	are smokers
Of 66 fraternity men	49 or 74.2 per cent.	are smokers

There are more smokers among athletes and a great many more among fraternity men than among all students.

Of 223 students	84 or 37.6 per cent.	made varsity teams
Of 115 smokers	47 or 41.0 per cent.	made varsity teams
Of 66 fraternity men	41 or 62.1 per cent.	made varsity teams

There are more athletes among smokers and a great many more among fraternity men than among all students.

	Average Marks at Entrance	Average Marks in first 2 years	Average failures in first 2 years
223 students	90 per cent.	66 per cent.	7 per cent.
115 smokers	89 per cent.	62 per cent.	10 per cent.
84 athletes	90 per cent.	63.2 per cent.	8.4 per cent.
66 fraternity men	85.4 per cent.	59.1 per cent.	12.8 per cent.

Smokers, athletes and fraternity men have lower scholarship records than other students.

There is some definite relation existing between smoking, participation in athletics, membership in college fraternities and low scholarship. These relations indicate that the factor of smoking can not be isolated from other related factors which may account for differences in age and scholarship. It is very clear, however, that students who use tobacco invariably rank lower in scholarship than students who do not smoke.

Those who are conversant with present conditions in American colleges, recognize two distinct types of students. President Butler, in his annual report for 1908–09, devotes several pages to a discussion of this subject; among other things he says:

> Not so many years ago there were few boys who went to college without a serious, definite purpose more or less scholarly in character. They were looking forward to the ministry, to teaching or to the practise of law or medicine. Not many of them had in mind a career as merchant, financier or corporation official. With the lapse of time and the increasing wealth of this country, this condition has been very much changed. It is now fashionable to go to college, at least to some colleges, and the attractions of college life and companionship are powerful motives in leading young men to strive to surmount the barrier of college admission. This new type of college student, whether he knows it or not, goes to college primarily for a social, not for an intellectual, purpose. His wish is to share in the attractive associations of an American college; he desires to participate in athletic sports; he hopes in after life to mingle freely and on terms of equality with college-bred men. It is a good thing that boys of this type should go to college, provided that the college will recognize their existence as a type and will deal with them accordingly. To try to turn such men into scholars is a hopeless task. They are not fitted for high scholarship and they do not desire it.

The type of student referred to by Dr. Butler is a good fellow, he dresses well, has a generous allowance, belongs to a fraternity and tries to "make" some varsity team; he elects courses partly because they are easy and partly because the instructor is popular; he spends much time in social intercourse and athletics, and gets few high marks, mainly because he does not try to get them. This is the student who smokes, because he has the time, the money and the opportunities to indulge in the practise.

The non-smoker usually belongs to another type of student. He is the scholar who is ambitious for rank. Many students of this type earn part or all of their expenses by tutoring and other remunerative work; many of them hold free scholarships and must maintain high rank in their studies to retain them. Students of this type have little time for athletic training or social life of fraternities, and therefore few opportunities and incentives for indulging in the practise of smoking.

There are three points of interest brought out by this study:

1. College students who acquired the smoking habit before entering college are about eight months older at entrance than the non-smokers. Three factors are probably responsible for this difference in age: (*a*) all scientists who have studied the physiological effects of tobacco upon man and animals are agreed that it has a depressing influence upon the heart and circulation, also, that anything which interferes with the vigor of the circulation has a retarding effect upon growth. It is therefore possible that smoking may retard both physical and mental development; (*b*) the age seventeen is the time when most boys begin to smoke, if for any reason a boy is older than the average when he enters college, there is more than an even chance that he will have acquired the smoking habit in the secondary school, and (*c*) the type of student described above who is primarily interested in social life and athletics, is found in secondary schools as well as in college; three out of four of such students smoke, and they are usually graded low in their studies, these facts would account for a higher average age among entering freshmen who are smokers.

2. The physical measurements of freshman smokers are slightly above those of the non-smokers, and the smokers gain more than the non-smokers during the first two years in college, except in lung capacity. These figures are susceptible of misinterpretation unless three important facts are taken into consideration. (1) The smokers are 8 months older than the non-smokers; their measurements should be slightly larger on that account. (2) It was shown that smokers belong to a class of students having larger means and therefore a more favorable physical environment—better nutrition, etc.—than the non-smokers; th[illegible]urements should be larger on that account. (3) It was shown tha[illegible] participate in athletic exercises more than the non-smok-

ers; their measurements should be larger on that account. That the smokers are not appreciably heavier, taller and stronger than the non-smokers may be due to the depressing influence of nicotine on the circulation and the consequent interference with normal growth.

3. The scholarship standing of smokers is distinctly lower than that of non-smokers. The intimate connection existing between the smoking habit and participation in the social and athletic activities of college life makes it impossible to determine how much, if any direct influence the smoking habit exerts upon scholarship, but the results of this study and the similar results obtained at Clark College indicate very clearly that the smoking habit is closely associated with idleness and lack of ambition for scholarly achievement.

Conclusions.—The writer has no desire to defend the use of tobacco. The motive in making this study was to ascertain the facts concerning the effects of tobacco upon college men. The teaching of hygiene is making rapid progress; quantities of new books are being published in which the large volume of new scientific facts on nutrition, muscular exercise, and the effects of alcohol take the place of the dogmatic statements and easy moral of the old books; a similar change is desired in the treatment of the problem of the effects of tobacco.

A study of the literature on the effects of smoking, years of medical examinations of boys and men, experience in teaching hygiene and the results of this study have led the writer to the following conclusions:

1. All scientists are agreed that the use of tobacco by adolescents is injurious; parents, teachers and physicians should strive earnestly to warn youths against its use.

2. There is no scientific evidence that the moderate use of tobacco by healthy mature men produces any beneficial or injurious physical effects that can be measured.

3. There is an abundance of evidence that tobacco produces injurious effects on (*a*) certain individuals suffering from various nervous affections; (*b*) persons with an idiosyncrasy against tobacco; (*c*) all persons who use it excessively.

4. It has been shown conclusively in this study and also by Mr. Clarke that the use of tobacco by college students is closely associated with idleness, lack of ambition, lack of application, and low scholarship.

THE DANGER OF UNSKILL

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TWO human streams pour ceaselessly into the sea of American industry. One of these brings to us the immigrant, the man of foreign stock, alien in blood and customs, and more and more from the backward and "beaten" peoples of eastern Europe. The sources of the other stream are in our own life, and upon it are borne America's own children who, in the passing of years, are to face the duties of manhood and womanhood. These two streams fill the vast national reservoir of labor upon which depends in large measure the future of American industry and American moral welfare. This is the first fact to which attention is directed.

The second fact is the changing character of industry, aside from its human element. We are in the midst of the great mechanical revolution whose beginning in America goes back to the early years of the nineteenth century, but which since the civil war has been uprooting the old order, supplanting its simpler methods with marvelous rapidity and tremendous power.

The human consequence of this revolution is the driving out of the man by the machine, on the one hand, and the increasing specialization of labor on the other. And the labor supplanted by the machine, if it is to fit into the resulting more specialized employments, must have skill. Primitive man was unspecialized and his skill was of the slightest, his knowledge being insignificant. The man of to-day finds that sheer muscle is at a discount, and his weaker but better trained fellow passes him in the race. It is not meant that there is not a great demand for unskilled labor, but the unskilled laborer works under a constantly growing handicap.

In our earlier national history, it was possible for us to rely for prosperity upon the resources of nature. Force of body and character sufficient to brave the hardships of a raw and untrained world, and to pluck from nature the bounties which she furnished in abundance, was the quality most essential. Each man or family was a unit in production; cooperation or combination on any extended scale involving training, was not found or needed. Individualism and the overthrow of nature, and her exploitation, were the important features of our national life which assured success; and it was just these qualities of endurance, courage, force, assertiveness, aided by sheer muscle, which the selective

process of our early immigration brought to us. Only men and women of such qualities could and would face the long and dreary sea voyage and brave the peril of the unknown new world. Only the man of hope, of ambition, poor in the wealth of the world, but rich in determination, force and foresight, was suited for such migration. So too, it often was the leader of the advance movement of civilization in Europe who, because of political oppression, led a vanguard of the best blood of his country to share the bounties of nature in America.

But the day in which we can rely for prosperity upon nature's bounty is past. Her resources have been explored and divided up. And while new resources continue to be brought to light, they are the possession of the few, and offer little of hope to the hungry immigrant from the old world.

We can not, therefore, depend exclusively upon nature and the raw force and determination of our people to maintain or continue the old-time progress and high position of America. More and more our dependence must be placed upon ourselves rather than upon nature alone, and in particular upon a character acquired through training. The new industrial life, it has been said, demands skill. If America is to advance in industry, she must face this demand; her people must be trained and trained industrially.

If such is a true statement of the general character of the productive process of to-day, it is pertinent to inquire if the two streams of humanity, which furnish the labor necessary to production, are fitted to the more specialized demands of this process. Is our labor skilled? And what are its means of attaining skill?

Let us consider first the stream of immigration. The report of the commissioner general of immigration for 1907 shows that out of the total number of 1,285,000 coming to this country from other parts of the world in the year 1906, about eighty-three per cent. were without skill requisite to enter a skilled industry. If we eliminate from this number the women, children, aged and such other persons as are described as having no occupation at all, there remains fifty-nine per cent. of the total who are of industrial age and sex and yet are distinctly unskilled laborers. A large number, too, of those excluded are women who will enter unskilled trades, and many are children who will begin to earn at the earliest possible time in unskilled employments.

The fact that such a large proportion of the immigrant population is unskilled is inevitable. It is necessary only to recall that the great influx of the present and recent past is from central and southern Europe, from regions in which the opportunity to acquire skill is comparatively slight, and where the call for skill is not yet dominant.

If it be agreed, then, that the stream of immigration is pouring a mass of unskilled labor into our country, consider what is the case in

regard to the second source of our industrial life. What is the tendency to skill and the opportunity to acquire it among our own children who must soon enter industry? It is impossible to state this problem in a statistical fashion; but a fair idea may be obtained from a study of the industrial situation. Skill may be gained through two, and only two, methods. It must come either in connection with industry itself or in some way of preparation outside it; either through a system of apprenticeship or by way of vocational schools or school studies. In the older state of industry, the apprentice system of the guilds constituted a logical and efficient method of training. Boys became skilled workers under direction of a master and in the actual work of production. The apprentice system was the great industrial school of the past, and not only because it led to industrial skill, but also because it gave at least something of that mental discipline and power which we associate with the idea of a school.

This system, as is well known, is largely a thing of the past. It is true that apprentices are now received in some industrial plants, but the number so received is entirely inadequate to furnish a supply of skilled labor for the many lines of trade and industry. It is enough to say that the modern factory with its great specialization, is not as a rule, willing to train its skilled workers. It wishes its workers to come to it already skilled.

If training can not be gained as a part of the actual productive process, may it be acquired outside that process? Or, to state it differently, does our school system give the members of the growing generation a training which fits them to enter the industrial life as skilled workers?

We have in this country a considerable and growing number of trade schools and technical schools. We also find evening schools where vocational training may be obtained; and there are other opportunities of a similar sort. But it is not necessary to prove that there is but a scant beginning in this direction, as this is admitted by all students of the subject. It is clear that our present means of training for trade and industry through special schools is entirely inadequate, and it is equally well admitted that our common school system does not meet the need in this direction. Its curriculum has been determined by other interests than the economic needs of a constantly increasing industrial population.

In the excellent study by Professor Thorndike,[1] based upon returns from schools of twenty-three cities having a population of 25,000 or more, it is demonstrated beyond a doubt that the lack of opportunity for vocational training is a great cause of that heavy dropping out of school in early grades which thereby closes school education to a large

[1] "The Elimination of Pupils from School," p. 118 ff.

proportion of our children. Dr. Thorndike finds that only twenty-seven per cent. of those entering the first grade of the common school continue into the first year of the high school; and of these, thirty-seven per cent. drop out by the end of the first high-school year. The main cause of this enormous elimination from the high school has to do with the nature of the high-school course of study. Evidently a considerable number begin the high school at the age of fourteen or fifteen, an age at which little skill has been gained, yet which is favorable to its acquisition, but are discouraged by the lack of opportunity in this direction and so leave school altogether.

As is well known, it was found by the Massachusetts Commission on Industrial and Technical Education that "25,000 children between fourteen and sixteen years of age are at work or idle," that is, not in school; and the result of this careful investigation was to make entirely certain that these children had dropped out of school because they did not find there any possibility for training along lines which would prepare for the making of a livelihood.

We must conclude, therefore, that neither within the organization of industry itself, nor outside of it, in schools of any type, is there opportunity for the stream of growing boys and girls to gain in an economic manner that degree of vocational training which the conditions of modern industry demand.

What then is the situation which we face? First, the demand of our specialized commercial and industrial life for a larger and larger percentage of skilled workers. Secondly, a stream of foreign immigration pouring upon our shores an unskilled population much of which could not acquire skill readily, even if opportunity were presented, and which must inevitably supply largely the demand for unskilled labor. Third, a stream of growing boys and girls who must earn their living through our present complex and specialized forms of industry. Fourth, a comparatively slight chance of their gaining skill after they enter the industrial life, and no adequate opportunity to gain skill through the school before entering upon this work. What is the result? A demand for trained men and women, on the one hand, and on the other a vain beating against the bars which defend the skilled positions, by a mass of desponding, dissatisfied unskilled workers, with only the most venturesome and aggressive pushing through into skilled positions in a manner harmful and exhausting to themselves and weakening to the nation.

It is at this point that the real menace of unskill becomes clear. Much has been written and spoken about the retarding effect of unskill upon our national production, and this is indeed serious. But the real danger is more fundamental. Of greater importance than the product of labor is the worker himself. The effect upon our people of such a

situation as has been described, is the real danger. The problem is not primarily industrial but social. Unskill in the face of a demand for skill leads to degeneracy. In this fact lies its greatest menace. In his admirable study of "Misery and its Causes," Dr. Devine wisely suggests that the great cause of misery is maladjustment, and there is strong reason to think that his conclusion is correct. But just in so far as it is true that economic facts lie back of and condition the progress of civilization, to that extent failure to meet the fundamental economic facts involved in advancing stages of industry must constitute or lead to the greatest social maladjustment and consequent degradation and misery. It is maladjustment in respect to the most vital phase of life.

A great proportion of the young people of our country must enter an industrial calling. In what way does this unfitness for it affect their lives? The result is best shown by the often-quoted finding of the Massachusetts Commission on Industrial and Technical Education, for 1906. Out of 25,000 young people of from fourteen to sixteen years of age in that state not in school, it is reported that thirty-three per cent. were in absolutely unskilled trades and sixty-four per cent. in what are called low-grade industries, where the skill of the workers is very slight. Only less than two per cent. had found their way into really skilled industries. What does it mean, humanly speaking, to have a child employed in an unskilled industry? Simply that the child usually has come to the end of its development. On the side of industry it means a permanently small production and low earning power; on the side of the individual life, it means a stagnant mind and the consequences which flow from it. For it is not true that children remain in these low-grade occupations for a brief time, and from them pass to higher and more skilled employment. The nature of industrial and commercial technic is such that there is a chasm between unskilled and skilled employments. There is no passage from one to the other. The elevator boy or messenger boy is not being trained to be a mechanic or a telegrapher or any other more or less skilled worker. These and other low-paid juvenile employments represent a class of work of a special sort from which there is no exit and which rather unfit than fit one for better work. In the street trades, in candy-making, in cotton, woolen, knitting and other mill work, and in many other places such work is found. To a considerable extent it is work which should be done by machines and not by growing boys and girls. The child who leaves school to enter one of these positions, condemns himself in the majority of cases to an unskilled life. He passes from one unskilled position to another, becoming more and more discontented as he finds it impossible to advance in wages and responsibility. Discontent, [illegible]pelessness, shiftlessness, take the place of ambition and progressive force. The

unskilled employment is not disciplinary and it does not lead to a skilled employment which is disciplinary. In the organization of industry, the avoidance of waste is a great aim; yet the lessening of the greatest of all wastes—the waste of life—receives scanty attention.

The writer of "The Long Day,"[2] in drawing upon her own experience as an unskilled girl, looking for employment in a great city, summarizes the situation in these works:

> For sad and terrible though it be, the truth is that the majority of "unfortunates," whether of the specifically criminal or of the prostitute class, are what they are, not because they are inherently vicious, but because they were failures as workers and wage earners. They were failures as such, primarily, for no other reason than that they did not like to work. And they did not like to work, not because they are lazy—they are anything but lazy—but because they did not know how to work.

And again the same writer records her conclusions in regard to the educational need of girls in view of the modern demand for skill:

> And there are other things more important than the "three R's" which she should be taught. She should be taught how to work—how to work intelligently. She should be trained young in the fundamental race activities, in the natural human instincts for making something with the hands or of doing something with the hands, and of taking infinite pleasure in making it perfect, in doing it well.[3]

And it may be added that what is true of girls is equally true of boys. The great cause of failure and resulting degeneracy is lack of training.

It must be recognized that the vocational impulse is deep-seated, and as the child advances into youth he begins to look to the doing of his life's work. He is restless with simply academic subjects, however valuable. He is concrete in his demands. He wishes to do and earn. But it is an interest in the deep human instincts and forces which must be laid hold of, if we are to develop a healthy, hopeful life; and among these we must recognize the economic instinct leading to the desire to earn and to make a place in the world of production. How much of progress flowed from the development resulting from the vocational education of the apprentice of the guild organization, it is not possible to say; but it certainly was a factor of no small import. And the close association of the wonderful expression of artistic genius in Italy with the development of the skilled artisan and craftsman, is a feature of social history which should lead to serious reflection.

But, further, lack of skill means insecurity of employment for adult workers; and no greater danger threatens labor than this. Every slackening of trade, every depression of business, every interference with industrial progress, every mistake of judgment of the organizers of industry, falls with heaviest force upon the unskilled. Their value in

[2] Page 277.
[3] Page 294.

industry is least, their tenure of employment is most easily imperilled. The past two winters with armies of unemployed in every large city, recruited largely, we are told by competent observers, from the unskilled, bear witness to this fact.

A consequence of economic insecurity is a weakening of moral tone and grip; this is the greatest of all dangers to society. "Every great industrial crisis leaves behind it," says Dr. Warner, "a legacy of individual degeneracy and personal unthrift."[4] "Involuntary idleness intensifies and perpetuates incapacity." Nothing so begets failure as the consciousness of failure. The discipline of regular and continuous occupation is a support which few can do without. At the recent meeting of the British Association for the Advancement of Science, a member of the Royal Commission on the Poor Laws held that pauperism arises mainly from the casual worker class, that is, in the main, the unskilled class whose security of employment is slightest and whose mental attitude is therefore least hopeful and healthy. To live on the edge of social existence blinds the eyes to the social order which is not near the edge. Hopefulness of mind is a social force impossible to measure. It is hope which marks the difference between slavery and freedom, between stagnation and progress. But insecurity weakens and destroys hope, and if employment continues to be insecure, the result must be an increasing body of hopeless men and women, feeding, inevitably, the ranks of criminal and pauper degeneracy.

Viewed from this point, the significance of unskill becomes tremendous. Lack of skill stands as the bar to mental progress even in an unskilled age; but in an age demanding skill, the lack of it is itself a condition leading to degeneration. Through unskill, labor is condemned to low wages, a narrow outlook, an inability to meet the modern demands of industry; by remaining economically unfit, men become socially unfit and are forced for themselves and their children into the ceaseless round of struggle for bare subsistence, with consequent hopelessness, bodily decay and resultant misery. It should be clear that in refusing to meet the industrial needs of our age for skilled workers the nation is condemning a considerable part of its population to an inevitable economic unfitness and resultant mental sterility, since economic well-being is essential to mental stability and progress. Degeneracy, thus, is born of the unskilled hand and the untrained mind.

There is one further position which needs to be considered. It is becoming clear, as investigation into social life proceeds, that human progress depends largely upon society's creative minds, its "inventors," its originators, whose fertile ideas are passed on to the mind of the mass of mankind. It is these suggestive and fruitful ideas which mark the stages of advancement and which constitute the essence of civilization.

[4] A. G. Warner, "American Charities," pp. 103 and 97.

And it may be said, further, to be a matter of at least large probability that these creative minds may be brought forth in any stratum of society. Whether they shall develop and give to civilization the benefit of their talent, depends upon the conditions surrounding them. They may grow and become mentally fruitful, or be repressed and become sterile, according as social environment is favorable or the contrary. It would seem that society should make every effort, in its own interest, to encourage their nurture and preservation. But, as Dr. Ward has so well shown,[5] education is the greatest social agency for providing that the mind, strong by nature, shall develop and give its ideas to the world. How great therefore is the urgency that society should afford educational opportunity to all classes of its people. How great a part of the possible progress of the race or nation is hindered by the social waste of its creative ability which never arrives at its period of fertile productiveness for lack of suitable social opportunity.

It should, however, be clear from what has already been said that the only education which can reach the masses of a nation and hold them long enough to be of educational service to them, is that which looks toward vocation. And it therefore follows that only by making our school system, to some degree, industrial and vocational, and thereby holding our children under educational influences for a longer period, can the great number of productive minds, born in poverty or other unfavorable conditions, be preserved and brought to that stage of development in which they may advance the nation.

Here, then, is the real danger of unskill. Modern industry calls for skill. In the face of this demand, lack of skill leads to unemployment and so to social weakness. Lack of skill leads, also, to poor employment; and so, likewise, carries men into shiftlessness, discontent and degeneration. On the other hand, skill breeds hope and hence mental development. It opens new avenues of activity and draws out otherwise buried talent, and thus preserves the originators to the race. But our two streams of labor are inadequately trained for the economic demand. What we should do in regard to the stream of immigrants is a problem by itself. But as for our own children, the demand for opportunity to gain that skill, which will enable them to fit the economic life of to-day, is a very urgent and vital one.

[5] "Applied Sociology," chapter X.

BACTERIOLOGY AND PARASITOLOGY IN RELATION TO AVIAN DISEASES

By GEO. EDWARD GAGE, Ph.D.

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DURING the last ten years the sciences of bacteriology and parasitology have been established beyond previous expectation. To-day these sciences are so far-reaching that they not only have to do with medicine but extend into the realm of hygiene, agricultural sciences and the industrial arts. The advances made in connection with the life histories of the various microscopic animal parasites and the studies which have led to a general understanding of the relation between parasite and host have done much toward unearthing mysteries of diseases which attack domestic fowls and menace the poultry industry. Bacteriologists have enabled the avian pathologist to study and control these fatal diseases.

Practical applications of bacteriology to the arts and industries are only instances of the ramification of this science. In agriculture and closely allied science, bacteriology and also parasitology have been immediately and intelligently employed to set forth new facts and expose new problems. During the last few years bacteriology has held close relations with medical science. By the application and extension into the field of protozoon pathology one of the latest and most helpful developments in the study of infectious diseases has evolved. This is not alone true of human pathology, but must include avian, insect (such as bee and silkworm), sheep, swine and cattle diseases and possibly the diseases of plants.

In the poultry yards epidemics of the so-called "white diarrhœa," "black-head" of turkeys, and tape-worms, have demanded scientific study for remedial help. The loss to the poultryman is at present almost incalculable. The etiology of many diseases is understood only by the discovery of some bacteria or parasite. The mode of entrance of the invading microorganisms to the avian body, the study of the original source of the infectious material and the possibilities of transmission and infection can be apprehended only through prosecution of detailed bacteriological and parasitological studies. Individual birds may suffer from malnutrition and be afflicted with ailments which may be the result of inability to utilize food properly, but when a whole flock becomes droopy. listless and unable to maintain normal life, we must resort to the field of parasitology or bacteriology for the cause.

In the warfare against the ravages of disease a most rational hygiene of the poultry yards must be observed, and in order to understand thoroughly those factors which have to do primarily with eliminating the trouble, it is to the use of disinfectants and antiseptics that we must resort. Here again the science of bacteriology lends a helping hand, for data concerning the efficiency of disinfectants can be ascertained only by bacteriological technique. It then becomes the duty of the scientist to direct his entire attention to those factors which in themselves are sufficient to allow a foothold for many an infectious disease. It is no less a fact among domestic birds than with human beings, that infection may take place by contaminated food, the particular parasite or organism being transmitted in such manner. All the modes of spread are recognized, and just as the spread of human diseases are held to be matters of public concern and preventive measures are instituted by expert bacteriologists, so also should the spread of diseases among domestic fowls be of the same great concern to the poultryman if he is desirous of maintaining his birds in a healthy condition.

Probably one of the most difficult problems in relation to avian diseases lies in the prompt recognition of the cause, so that measures may be employed immediately to allay the trouble. With the large poultry farms it may appear that careful observation of hygienic measures involve too much time, but under many circumstances and especially at this infant stage in our knowledge concerning avian diseases the application of searching and delicate parasitological and bacteriological tests are often necessary to determine the proper method of procedure.

The great losses to poultrymen from the disease known as "black head" or "coccidiosis of turkeys" has called scientific men to make thorough investigation and a specific parasite known as a coccidium has been claimed to be the cause. Dr. Geo. B. Morse, of the Bureau of Animal Industry, United States Department of Agriculture,[1] states that this coccidium may infect turkeys, ducks and pigeons. It has a definite life cycle. He describes it as a certain circular, sometimes slightly oval, cyst, 12 to 25 microns in diameter, containing granular matter which may fill the cyst or occupy only a portion of it. These are permanent cysts and may be voided in the feces of the bird. These only require warmth and moisture for their development into sporozoites by which the disease is transmitted to other birds. By the destruction of the malarial parasite within the body of man, we may break the life cycle and thus interrupt the continuity of the transfer between man and mosquito in the transmission of this disease. In like manner, the scientist can plan to break the cycle of these avian parasites within the

[1] Circular 128 (1908), Bureau of Animal Industry, Dept. of Agriculture.

body of the bird and consequently eliminate infection. Contributions from the Division of Biology of the Rhode Island Experiment Station have furnished us interesting facts concerning parasitism of *Cytodites nudus,* a mite and *Hæmaphysalis chordeilis,* a tick and these are but a beginning to the study of such parasites affecting birds. It demonstrates the field for research in parasitology and what contributions from this realm of science would mean in determining the cause of so many diseases, the etiology of which at the present time is unknown.

Fowl typhoid, cholera, tuberculosis and hosts of other afflictions were discovered through the aid of scientific bacteriologists. In a very recent publication[2] Professor Rettger, of Yale University, has demonstrated the value of bacteriology, by his valuable contribution to the study of white diarrhœa. He has been able to demonstrate the rôle of bacteria in the etiology of this disease. We need no better example of the usefulness of such a science in planning investigations of this nature. By thorough bacteriological methods he has been able to give us the results of his work and has shown how infection may occur, what it means to the poultry industry, and methods of prevention. This also demonstrates how bacteriological methods have been used to study epidemiology. It has given a procedure based on bacteriological facts and with such methods at hand we are supplied with the means of suggesting treatments which undoubtedly will do much toward solving the problems which have heretofore been unsolved. These studies have shown that the function of pure water and food and sanitary conditions are essential to the daily life of domestic birds. If diseases of the poultry yards are to be suppressed, hygienic measures must be observed here as with human beings. It was not until after the introduction of hygienic measures such as a proper sewage disposal, and water filtration that the death rate of typhoid fever was perceptibly diminished in this country and Europe.

Conspicuous as the achievements have been in bacteriology, it can not be said that the field is exhausted. There is hardly an infectious disease of the poultry yards which does not have to do with some bacterium or parasite, and the variations and adaptations of these pathogenic forms is to-day one of the difficult problems with which the avian pathologist has to deal. It is for the scientist to determine whether certain bacteria and parasites owe their pathogenic action to the organisms themselves or to their toxic or poisonous by-products. The field of immunity as related to avian pathology is unexplored. This would be among the most complicated that the scientist could undertake, yet the fields of bacteriology and parasitology with its many perfected methods of attack would indicate that it is not impossible. Not only human medicine, but also veterinary science owe much of their ad-

[2] Bulletin 60, Conn. Agr. Exp. Station.

vancement to these two fields of knowledge. The scientific contributions of Neuman on parasites show that such organisms are the cause of many a dreadful disease, not only with man and animal, but with all avian life. The careful study of their life histories should appeal to our protozoologists and inspire them to contribute to our knowledge of those parasites which are causing an enormous mortality of our most valuable birds.

After the removal of a sick fowl from the flock a diagnosis is usually made. Very often the specific organism causing the infection is readily discovered, while, on the other hand, the most diligent scientific efforts may fail to reveal the character of the disease. Many failures are accountable because there is a lack of sufficient knowledge or a lack of thoroughness in making the investigation.

The fundamental difficulty in ascertaining more definite knowledge about our poultry diseases is the lack of enough scientific men to take hold of the situation. To-day we have no rational system of medical treatment for birds, nor can one be looked for until scientists, who are busy on anatomy and physiology of avian life, offer to the layman a definite plan of procedure, when these parasites and bacteria have made their way into the body and brought about pathological lesions.

The relation of bacteriology and parasitology to the infectious avian diseases as mentioned before, is fundamental. If bacteriologists and protozoologists will enter upon this field of avian diseases as a basis for their research in their respective fields, the results of their investigations will lead to an improvement in the conditions of our poultry yards, and give facts which are necessary before any treatment can be found. With men of this character at work upon avian pathology, success is inevitable.

THE RÔLE OF SELECTION IN PLANT BREEDING[1]

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WHEN one attempts to give some idea of the principles and of the methods and scope of plant breeding, the matter falls naturally into two parts, the rôle of selection and the rôle of hybridization. This is not because the subject is really thus separable, but because the methods in use fall into these categories. One must, of course, use selection after hybridization, but there are a number of plants of great agricultural value, in which either the flowers are too small for artificial crossing or in which other reasons make it desirable to use simply selection in their improvement. It is of these that this paper will treat.

The particular work discussed has been selected because it will illustrate certain principles, not because it is regarded as more important than other work of like nature. The work of many quiet men who are striving for the good of mankind by their efforts toward the improvement of plants deserves to be mentioned, but unfortunately the limits of a single paper are too narrow to discuss principles and to say much about practise, and knowledge of the former should be made more widespread in order that the latter may be appreciated.

The non-botanical public can not be blamed if it receives comparatively worthless productions with greater acclaim than those of value when the former obtain all the publicity and no voice is raised in protest. Exploitations of new plant introductions of little value have certainly been numerous in the past few years. Perhaps this has been a public benefit, for it has increased the general interest in plant breeding and has stimulated many laymen to study the subject in order to be able to separate the wheat from the tares when dealing with new varieties. It is strange, perhaps, with our reputation for always looking for the dollar sign, that the new agricultural productions of greatest economic value have always received less notoriety than the production of horticultural novelties of limited use and small importance; yet such is the case. It is doubtful whether the production of a new field corn that would increase the yield in the United States by ten per cent. would obtain more than a passing notice from the press; yet such an increase would add $100,000,000 per annum to the wealth of

[1] This paper is based on a series of poular lectures delivered at the Bussey Institution of Harvard University April and May, 1910. A second paper will follow.

the country, and the individual who was responsible would deserve to be ranked among the greatest benefactors of the commonwealth.

This illustration serves to show something of the extent of the benefits that may be confidently expected from the improvement of cultivated plants; but the full extent of our rightful expectations is at least ten per cent. increase in both quality and quantity of all the great crops of the United States. In fact this is a very conservative forecast based upon what has been accomplished in the past. Men like Haynes with his "Blue stem" wheat and J. S. Leaming with his "Leaming" corn have perhaps made an even greater percentage increase in the value of the returns from the land upon which their productions have been grown. Their results were obtained largely in the latter half of the last century and even greater advances should be made in the future. This statement is made because, in the last quarter of the nineteenth century, experimental biology was in the same relative position in which chemistry stood in its beginning. During the century chemistry made wonderful advances; during this—the twentieth—century experimental biology will make similar progress. And one of the first and most important applications of the facts discovered will be to guide and direct man in producing new plants and animals by more direct and certain methods.

When one speaks of producing new plants, however, he should not be misunderstood. Man has not yet actually produced new variations (although the time may come when even this is possible); he simply works with the variations which have occurred through natural causes of which little is known. The isolation of a varying plant and from it the production of a variety, or the combination of desirable characters from one strain with other desirable characters from different strains, comprises the total aim and desire of the plant breeder. The idea is simple; to put the idea into practise successfully is often a tedious and difficult task.

As in hybridization the ease with which results can be obtained by selection depends largely upon flower structure. In selection, however, the relative facility with which artificial cross-pollination can be accomplished is of small importance. What one wishes to know is whether cross-pollination or self-pollination takes place naturally. Practically all plants are occasionally cross-fertilized naturally, and many of them have devices whereby they are nearly always crossed; but we are coming to see that cross-fertilization is not as essential to plant life as Darwin endeavored to prove in his "Cross- and Self-fertilization in the Vegetable Kingdom." Wheat, for example, is almost always self-fertilized; yet it has kept its vigor for thousands of years. The importance of this fact to the selectionist is easily seen. If seed from several varieties of wheat is mixed and planted, each variety remains

true to its type because of self-pollination, and during the growing season the plants can be compared and any desirable type selected for future propagation. In a cross-pollinated plant like maize this is not the case. The pollen is carried by the wind through long distances and varieties planted close together are continually intercrossed. The isolation of a particular type is not simple as in the case of wheat, but may be prolonged through many generations. Each prize ear selected for future planting will have had at least a few and possibly many of its seeds fertilized by pollen from less desirable strains. When these seeds are grown they of course again fertilize the seeds of the desirable plants with a frequency proportionate to their number.

In the case just cited recourse may be had to artificial self-pollination. Several hundred seeds are thus produced at one operation and the work of isolating the new variety is made materially easier. But suppose we are dealing with red clover where the flowers are small, almost sterile with their own pollen and produce only one seed. In this crop, the long and tedious method of continuous selection just mentioned must be used, for there is no other way. This method is often called the pedigree-culture method. The main idea of the plan is that the seeds of *single* plants are grown in isolated plots, and the character of the mother plant judged by the characters of the progeny. This method has given much better results than the so-called German method, which consisted in planting a mixed lot of seeds from several of the best plants. For example, the German sugar-beet raisers have for years analyzed large numbers of sugar-beets and have grown their seed from the mother beets showing the highest percentage of sugar. No particular attention was paid to the general average of the progeny of each beet; those were bred from which appeared to be the best as shown by the polariscope sugar test. In this way the amount of sugar produced per acre was gradually increased, but progress was slow and cessation of selection immediately caused the sugar content to decline.

To see the real reason of this we must go back to the time of Darwin. The data from which Darwin proved the doctrine of descent came in large measure from domestic animals and cultivated plants. He saw that plants varied among themselves and that by selection of the variants new types were gradually produced. From these facts he argued that all evolution had taken place by the selection of minute variations and generally through the selective agency of a contest for life taking place among all living organisms. This he called the agency of natural selection. Later, however, Bateson, Korshinsky and de Vries called attention to the fact that many new types of animals and plants are known to have originated suddenly. There was no gradual evolution of the type; it simply appeared fully formed. This hypothesis, called the "mutation theory," found great favor among plant breeders

for they knew that many times they had noticed and isolated plants showing new characters from their cultures, and had carefully made selections for further improvement of the new strain, but that generation after generation showed no further progress. LeCouteur, whom de Vries cites as the first known user of the pedigree culture method, had a case in point. From the heterogeneous lot of wheat plants which he was growing, he isolated a uniform type of great merit which he called "Bellevue de Talavera." For years after, this strain was subjected to selection in order to bring about further improvement, but the efforts were made in vain, for no new heritable variations were produced. Yet something was lacking from this theory. Sometimes there did appear to be a gradual improvement by selection. De Vries said that this was merely a temporary improvement made by selection of quantitative variations. He believed that when selection ceased, sooner or later the improved types would return to the original type of the variety from which it had been produced. The real interpretation of the facts and one which fitted all the parts of the puzzle together, came from the work of Johannsen and later investigators. It is an explanation that should have been thought of before, but like many other important discoveries, it was too simple for ordinary minds to grasp. Weismann had shown years before that the inheritance of characters acquired through outside influences during the development of the body was probably mythical. His investigations led him to believe that there is a continuity between the reproductive or germ cells of different generations, and that the body is nothing but a temporary house built to shelter them. Injuries to the house have no effect on the future generations unless the germ cells themselves are affected. Later Boveri and others, through their cytological studies, showed that the future germ cells are laid down at a very early stage in certain animal organisms and that very few cell divisions take place before the maturation of the reproductive organs and the production of active germ cells. The body cells he found to be built up by continuous cell division of a very different part of the original fertilized egg. Since no biologist, however, had found or is likely to find similar cytological phenomena in plants, no one seemed to grasp the idea that here was the key to the question that had been puzzling the plant breeders. Johannsen, however, brought matters straight by his experiments on beans. He found that commercial varieties of beans, though pure in grosser characters, such as color, were actually very mixed types when such characters as length or weight were studied. Several investigations were undertaken on size characters, the characters most rapidly affected by changes in environment. He found that his commercial variety fluctuated around an average size and that when seeds larger or smaller than this type were selected they responded to it in whichever direction the selection was made. The progeny of the

selected beans were not so extreme, however, as their parents but regressed toward the average character of the parent race. This was nothing new. Galton had discussed the matter a decade before and had interpreted the regression as due to the "pull toward mediocrity" exerted by former ancestors that must have been on the average mediocre. Johannsen was not satisfied with this interpretation and in order to investigate the subject more thoroughly introduced the individual pedigree culture method, or pure line method as he spoke of it, into his work. All of his plants under experiment were self-fertilized for successive generations, so that all of his future bean progeny were descendants of single individuals from the original commercial variety. Each pure line he found to fluctuate around a typical size just as the commercial variety had done. Some types were exactly the same as the original mixed type, but others fluctuated around averages that would have been considered more or less extreme variations in the original. He then grew extreme variants from each of his pure lines and made the discovery that no progress at all was made by repeated selections of this kind. The progeny of the high extremes and the progeny of the low extremes each were found to fluctuate around the same pure line average. It was quite evident then that in the first place he had been dealing with a mixed race. This mixture consisted of sub-races each with a heritable difference in the character size. These heritable variations, however, were obscured by size fluctuations produced by differences in moisture, sunlight and fertilizer received by the different individual plants. There was even a difference in the size of individual beans on the same plant, due probably to location of some pods in places on the plant more desirable than others for the utilization of the plant's soluble foods waiting to be stored in the seeds. *These differences due to immediate environment were not inherited.* They behaved exactly as the acquired characters of an animal. This made the rôle of selection clear. The only improvement that selection can achieve is to isolate a substrain if such a substrain or substrains exist in the variety under experiment. When this substrain has been isolated, selection has absolutely no effect, and even if continued for countless generations will have no effect until nature produces one of the heritable changes which are so much rarer than the fluctuations produced by environment. It is also evident that the older idea that improvements made by continued selection—*i. e.*, gradual isolation of a type—are inconstant, is wrong. The explanation is that since non-inherited fluctuations obscure the heritable variations, only a pure line method can absolutely isolate a pure strain; and in the German method of mass selection with poor control against mediocre pollen, the chances were overwhelmingly in favor of the selected type recrossing with the more commonly cultivated and poorer type from which it came.

To my mind this work should clear up the strife between the critics and the adherents of evolution by mutation. It is evident that there are variations that are inherited and variations that are not inherited. If we call the one a mutation and the other a fluctuation, we have a distinction that will stand analysis. Why should a further distinction be made? De Vries believes mutations to be qualitative, fluctuations quantitative. Nevertheless, quantitative changes that are transmissible occur in much greater numbers than do qualitative changes. Opponents of mutation believe wide jumps appear too seldom to have been a factor in organic evolution, but they can not deny that they do occur. There are too many authentic cases in variation under domestication. Yet no one who has had experience in breeding plants will deny that small variations (not fluctuations) occur with much greater frequency. While it is impossible to prove it, I believe that the mathematical law of error controls the transmissible variations as well as fluctuations. If one could collect a random sample of variations that are inherited he would probably find that a great many forces act as the causes, and therefore as in ordinary probability, the extreme changes—that is, the great variations—occur with less frequency. One should remember, however, that in our present state of physiological knowledge, he can not know with much certainty which of two changes that apparently differ greatly in magnitude is really the greater in the light of the plant's economy.

It might be well before leaving this part of the subject to speak of one other point. In a strain that has been self-fertilized for several generations, gradual progress has sometimes been made by selection. This probably comes about because the parent plant is still hybrid in regard to certain characters, and it is to their recombinations that the intensification or reduction of certain apparently single characters but which are really combinations of separately heritable characters, is due. According to the law of chance with repeated self-fertilizations any strain approaches a constant condition in all of its characters when unselected, but one can not say when this state is reached unless he knows the exact number of hybrid characters in the beginning and can recognize each.

If we were to take up the crops of the United States which owe their present excellence and future prospects in large measure to the isolation of superior strains by selection, we should cover a great majority of the agricultural wealth of the country. Of course natural cross-fertilization and even occasional artificial hybridization have played important parts by causing recombinations of characters, but selection has been the main cause of improvement. Two of the important crops, tobacco and wheat, are very seldom cross-pollinated naturally; nevertheless new types are continually appearing in the fields. To make new varieties

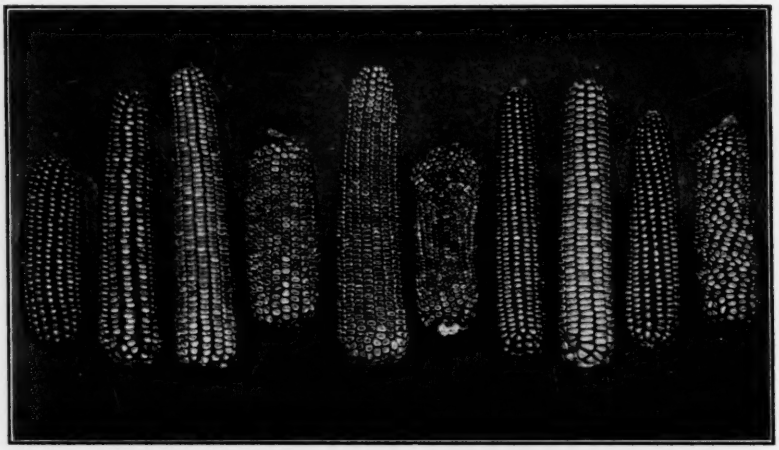


FIG. 1. TYPES APPEARING IN A SINGLE FIELD OF MAIZE. A strain like the ear near the center has been isolated.

it simply takes an alert eye for their detection, comparative tests to prove their merit and the time needed to produce a sufficient increase for commercial use. Some of our other important grain crops like oats and rye are more often cross-pollinated, as is also our chief grass crop, timothy. But as maize is probably the most difficult crop to deal with, and is a typical cross-pollinated plant as well as our most important cereal, perhaps it will be of interest to take a short survey of some of the problems with which one has to deal when endeavoring to improve it by selection.

Maize is the only one of our cereals that is monœcious. The tassel contains the pollen or male element while the silks are the stigmas of the female flowers. In order that the pollination of the silks shall be relatively certain, each tassel produces about thirty million pollen grains; and as the ears average less than five hundred seeds apiece, there are about sixty thousand pollen grains produced for each kernel. With such a large amount of superfluous pollen floating around in the air, there is a great deal of inter-crossing between the neighboring plants. This fact has been an obstacle to the improvement of maize, but it has been offset by one advantage it possesses over the other cereals, that of producing large ears. Since each individual ear must be handled and its characters noted at husking time, it is not strange that ears with desirable variations sufficiently striking to catch the eye of the grower have become the parents of numerous distinct varieties. By selecting desirable seed ears and isolating them from other varieties, various strains have been produced that are remarkably uniform in characters such as color that have forcibly attracted the attention of the breeder. Even in these strains, however, there are many natural types growing side by side and continually crossing with each other. There are stalks

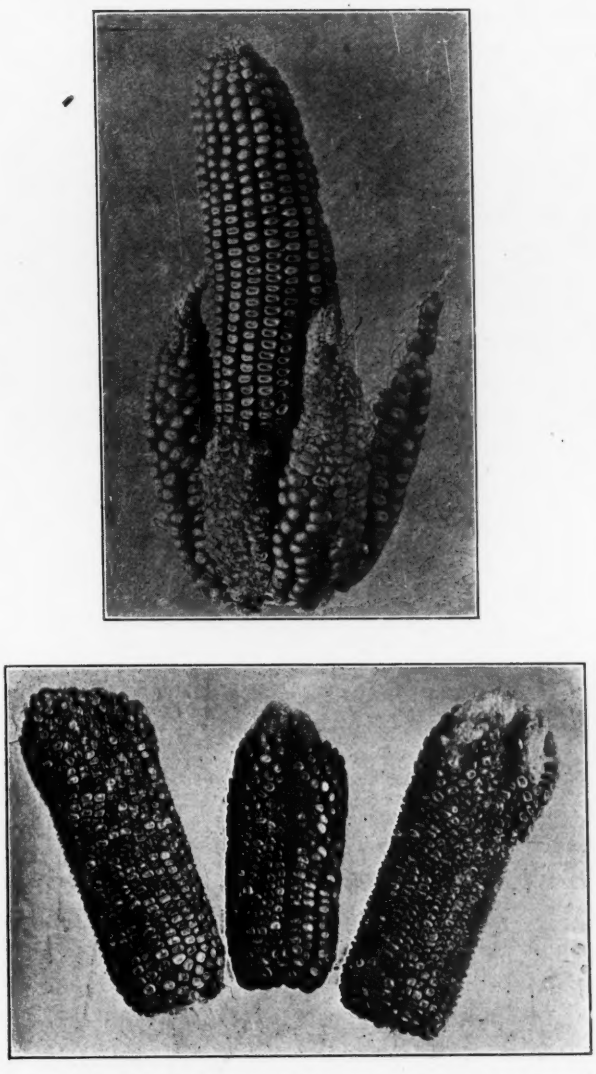


FIG. 2. INHERITED ABNORMALITIES APPEARING IN MAIZE. All pollen from these plants and from their normal sister plants must be prevented from maturing.

which bear their ears high and stalks which bear them low, stalks with long and stalks with short ear shanks, stalks with different leaf markings and with notably different tendencies to produce suckers. Differences are everywhere present even in the ears, as is shown in the accompanying photograph (Fig. 1). A large number of these differences are simply fluctuations produced by the environment and are not inherited. The obscuration of heritable variations by the fluctuations and the mixed condition of the natural types makes it a difficult task to isolate the most productive types. Many variations of technique have been proposed

for the prosecution of the work, but are all based upon the idea of proving the capacity of a mother ear by the characters of the progeny produced. If a very large number of ears are included in the original stock, it is unquestionable that some of them will transmit more desirable characters than others. It only remains to test them out by growing the seed of each ear in marked plots or rows and gradually eliminating the undesirable types.

The accompanying diagrams, showing the work of the Illinois Agricultural Experiment Station in their experiments in selecting for high and low protein content, and high and low oil content, admirably illustrate the rapidity with which progress can be made by selecting only from the maternal side, even in the face of constant intercrossing. This work the writer believes has given a com-

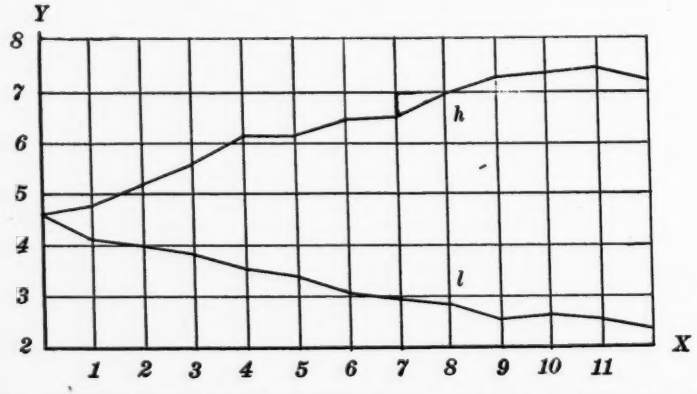


FIG. 3. DIAGRAMMATIC REPRESENTATION OF THE RESULTS OF THE ILLINOIS AGRICULTURAL EXPERIMENT STATION in selecting for high and for low protein content. *Y*, per cent. protein in crop; *X*, generations; *h*, high protein strain; *l*, low protein strain.

plete corroboration of Johannsen's conclusions on pure lines. This interpretation has been made, however, from their published data, and the Illinois station should not be held responsible. This work of breeding to change the composition of maize was started in 1896 with a hazy Darwinian idea that as corn was known to vary in composition, continuous selection of extreme variations would produce a continuous change in type. A very old type—Burr's White—furnished the foundation stock. A chemical analysis was made of parts of the individual ears each year, and the extreme ears planted. From the first, the four lines above mentioned were planted in isolated plots and were continually selected in the same direction. After ten generations the average crop of the high protein line had reached 14.26 per cent., while the low protein line was only 8.64 per cent.; the high oil strain had reached 7.37 per cent., while the low oil strain was reduced to 2.66 per cent. These facts clearly show the rapidity with which results can be obtained

by this method of selection even with a crop that is often cross-fertilized. But the diagrams show other facts. The published records show that the variability of the race was but little, if any, reduced by continuous selection. With extreme variants comparatively as far removed from each year's type, available for planting in each successive generation, the gain each year should have been at the same rate, if the Darwinian interpretation of the rôle of selection were correct. On the contrary, we notice that the regular curve fitted to the crop averages for ten generations, is first concave showing great progress made by selection, is later convex as progress becomes slower, and last becomes horizontal

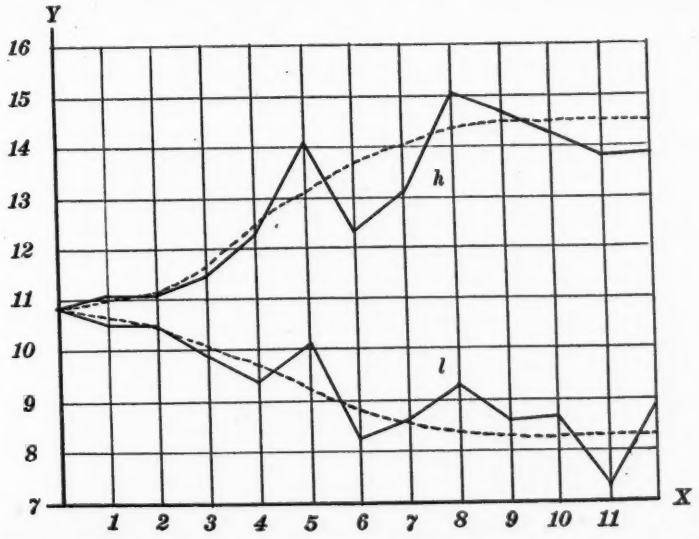


FIG. 4. DIAGRAMMATIC REPRESENTATION OF THE RESULTS OF THE ILLINOIS AGRICULTURAL EXPERIMENT STATION in selecting for high and for low oil content. *Y*, per cent. oil in crop; *X*, generations; *h*, high oil strain; *l*, low oil strain.

as no more progress results. It is very evident that the original stock was a mixed race containing sub-races of various composition intermingled by hybridization. Selection rapidly isolated these sub-races. The isolation was practically complete at the eighth generation in the case of the protein strains and the ninth generation in the oil strains. After this selection accomplished nothing. That the effect of selection was simply the isolation of a sub-race and not a continuous response, is further demonstrated by the fact that in 1903 another plot was started with seed from the isolated high oil strain. After four years' cessation of selection, the average composition of the crop remained the same, showing that after complete isolation of a homogeneous type no retrogression of the selected character occurs unless intercrossing with mediocre strains takes place. Fluctuation in composition still appears, but this is the non-inherited kind produced by external conditions.

It is sometimes somewhat difficult to see why selection of this kind should yield results slowly. There are indeed many points concerning which little is known. One may picture to himself, however, that where crossing is always likely to occur and where the apparent character is in reality a combination of a number of separately inherited characters, many thousands or even millions of individuals would have to be grown to run a fair chance of obtaining the most desirable combination. By growing a few individuals in which the desired character is intensified in successive generations, the combination wanted may be obtained with the use of smaller numbers.

I have stated that nothing can be accomplished by selection after a pure line or genotype as Johannsen calls them is isolated, unless a new transmissible variation is produced by nature. The questions then arise: how often may such changes be expected? and, what is their nature? Such changes are of two kinds,[2] progressive where a new character appears, or retrogressive where a character is lost. But little can be said as to their relative frequency. Undoubtedly some species are in a more unstable condition than others and give more of such variations, as de Vries has already suggested. On the other hand, certain unknown combinations of external conditions may favor germ-cell changes. They are both rare, the progressive changes being relatively much less frequent than the retrogressive changes, but they are sufficiently common for several to have come within the knowledge of every experienced breeder.

There is another type of variation much more closely related to changes occurring in "pure lines" than is generally supposed. I refer to what is commonly known as bud variation or vegetative sports. Retrogressive variations of this kind are probably no rarer than the same kind of changes occurring in pure lines. No authentic progressive variations (as distinguished from digressive) are known. In my own experience in growing eight hundred species and varieties of tuberous solanums (largely potato varieties), fifteen retrogressive variations have been noticed, and the changes that occurred were exactly like those occurring in seed-propagated strains.

The relative value of progressive and retrogressive variations is difficult to estimate. In organic evolution the former must have been far more valuable; commercially the latter are often of great worth. We may cite, for example, the great value of the bush or dwarf varieties of beans, peas and tomatoes that have originated as retrogressions.

[2] De Vries also gives a third kind, digressive variations, such as occur when a character previously possessed by but latent in the plant appears. This class is unnecessary. Digressive characters appear either through the loss of a complementary inhibiting factor or the gain of a complementary factor necessary for it to become active.

In closing I should like to call attention to a fact both of evolutionary and of commercial importance. The first generation of crosses between nearly related types generally grows more vigorously than the pure types themselves. If the fertility is not impaired, they even fruit more freely. This is undoubtedly the explanation of Burbank's quick-growing hybrid walnuts, but if they were self-pollinated and grown for another generation a large percentage of the progeny would lose this character. In naturally self-pollinated types like tobacco, one sees the phenomenon expressed as greater vigor in a cross; in a continually intercrossed species like maize the same thing is shown by a loss of vigor when the plants are self-pollinated. It is clear then that if pure strains of maize are gradually isolated by selection, by the same token they lose in vigor and productiveness. The original mixed strain may contain

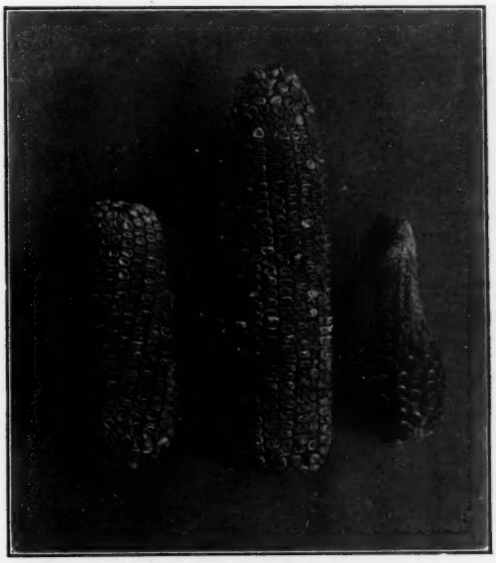


FIG 5. EFFECTS OF INBREEDING IN MAIZE. Outer ears inbred four generations. Middle ear the result of their crossing, first generation.

sub-strains some of which are much more productive than others. The less productive types may be discarded, but at the same time there is a loss of vigor from the fact that they are withdrawn from hybrid combinations. The logical procedure, then, is to isolate two high-yielding types, combine them by hybridization, and grow only the first generation of the cross. This is not mere theory, for by using such methods I have obtained from 100 to 200 bushels of shelled corn per acre on small plots. Unfortunately, this method can not be used to advantage on many crops, but in the case of maize the procedure is simple. There are many breeders using the isolation method of improvement. From

FIG. 6. RESULTS OF CROSSING TWO INBRED STRAINS OF MAIZE. At the right average of the parents after three generations inbreeding, 61 bushels per acre. At the left crop of first generation cross of the inbred strains, 101 bushels per acre.

them the grower obtains two strains and plants them in alternate rows. At flowering time all of the male flowers or tassels are removed from *one* of the plants of the varieties before they shed their pollen. All the ears that these plants produce are crossed with the other variety. It is this seed that produces the vigorous plants.

This method might be made the basis for some very valuable work in forestry. It is quite conceivable that many important timber trees might be found where nearly related species or varieties would cross readily. Experiment would show how great an increase in rapidity of growth could be expected, and whether such an increase would pay for the increased expense of hand hybridized seed.

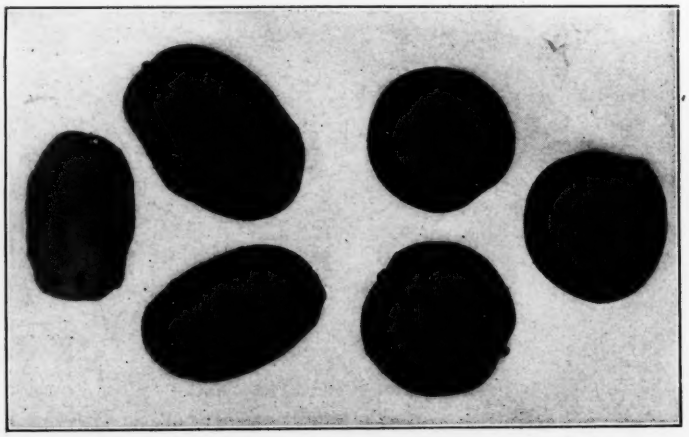


FIG. 7. SILVER HILL. Normal at left. Bud variation isolated by selection at the right.

One may summarize by saying that two important points cover the whole rôle of selection. The first point is that nature continually causes variations to appear in plants. The majority of these variations are simply accelerations or retardations of development of the whole or of certain parts of the plant due to good or bad environment at critical stages of the plant's growth. These variations are not inherited because the reproductive or germ cells are not affected. Other variations, however, are being constantly produced by nature—though much more rarely—which do affect the reproductive cells and are transmitted to the plant's progeny. These variations are the basis of selection. They are constant from the beginning and remain so unless changed by a second variation affecting the same constituent in the reproductive cells that is due to develop the character in question.

The second point to be remembered is that the whole aim and action of selection is to detect the desired heritable variants among the useful commercial plants and through them to isolate a race with the desired characters. When this is accomplished, selection can then do nothing until nature steps in and produces another desirable variation.

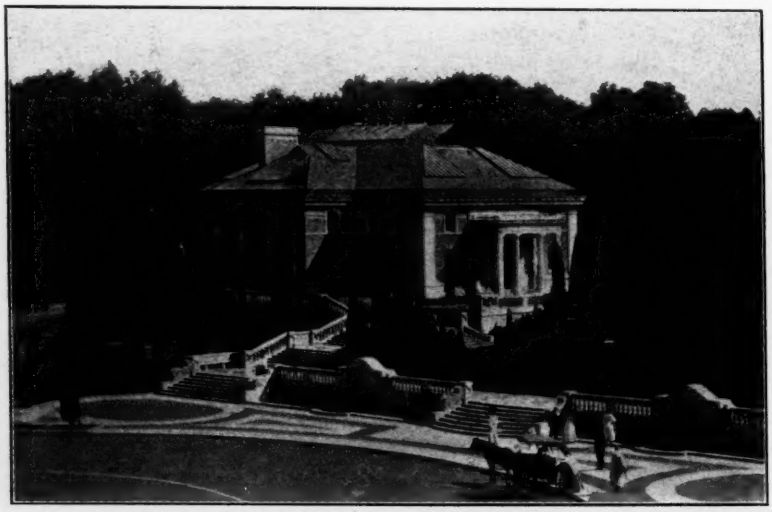
In other words, the results of selection are not continuous. Selection does not gradually perfect a character. The production of heritable variations is intermittent and the intermissions may be long. If the practical results seem to be parts of a continuous process, it is because of the imperfect methods at hand to isolate the desirable variations from their combinations with undesirable characters formed by natural hybridization.

THE PROGRESS OF SCIENCE

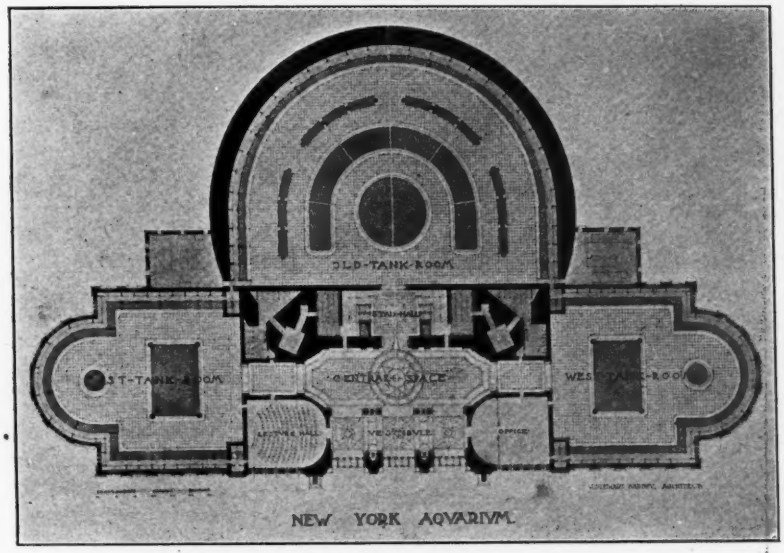
THE WORK OF THE NEW YORK ZOOLOGICAL SOCIETY

THE Zoological Society has performed an important service for the city of New York by the establishment and conduct of a Zoological Park and later by taking charge of the Aquarium. The relations of the society to the city are similar to those of the trustees of the American Museum of Natural History, of the Metropolitan Museum of Art and the Botanical Garden, but are somewhat unusual. In each case the city provides the buildings and the cost of maintenance, while a private corporation supplies the collections and is responsible for the conduct of the institution. The plan appears to have worked very well, as each institution has had a strong organization, free from any political control, but effective in obtaining large appropriations from the city and considerable private gifts.

The fourteenth annual report of the Zoological Society lays emphasis on increasing the scientific work done both at the park and the aquarium. The institutions have been extremely successful in gathering and maintaining large collections of animals and interesting the public in them; but they have not as yet been able to undertake research work comparable in value. The director of the aquarium writes in his report, "The small aquarium at Naples has made Naples famous." It is not, however, the exhibition tanks, but the research work and publications of the station which have added to the fame of Naples. The entertainment and instruction of the public is an important function for the city to undertake, and the money devoted to these purposes at the Zoological Park and the Aquarium is well spent. But money used for research is not spent at all; it is invested for the permanent benefit of all the people. Zoological gardens have hitherto emphasized scientific work less than have botanical gardens, but there are problems of comparative psychology and comparative pathology to which collections of



THE ADMINISTRATION BUILDING OF THE NEW YORK ZOOLOGICAL SOCIETY.



PRELIMINARY PLAN FOR THE ENLARGEMENT OF THE NEW YORK AQUARIUM.

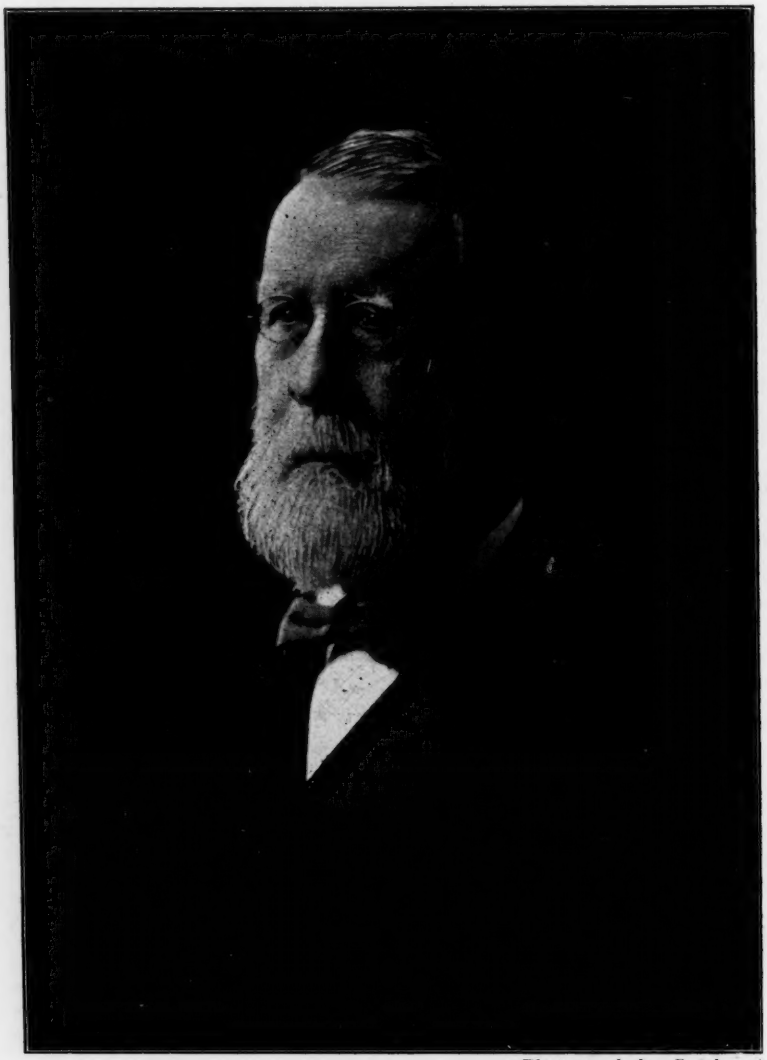
animals might be made to lend themselves admirably; and there are many kinds of research work in experimental morphology and heredity which might be carried on to advantage. While paying their cost in exhibits of general interest and unusual instructiveness to the public, they would at the same time advance science and its applications.

The report of the executive committee begins with the paragraph: "With this year closes the first period of the Zoological Park development, and from now on the work of the society will be, to an ever increasing degree, in the direction of the remaining objects of the society. Briefly stated, those objects are, scientific work in connection with the collections, and the protection and preservation of our native fauna." The director of the aquarium also urges the desirability of establishing a small staff of scientific curators. We may consequently expect that in a short time the contributions to science from the Zoological Park and the Aquarium will rival those from the Museum of Natural History and the Botanical Garden.

The director of the Zoological Park urges the need of additional bear dens, a zebra house and an aviary for eagles and vultures. He expresses the hope that these three buildings may be obtained during the present year and states that with these the animal buildings and other installations for exhibits will be practically complete. During the past year an administration building has been erected at a cost of $75 000. It is intended for executive offices and as a meeting place for the members, and is to contain a library and art gallery. At present a collection of some 600 heads and horns, in which the director has taken much interest, is housed in this building, but a separate building open to the public is planned.

The attendance at the park last year was 1,614,953, an increase of 200,000 over the preceding year. There were 5,000 animals on exhibition representing 1,117 species, of which 812 were mammals, 2,880 birds and 1,308 reptiles. This is an increase over 1908 of 155 species and 421 specimens, including many of special interest.

The attendance at the aquarium reached the remarkable record of 3,-



Photograph by Gutekunst.

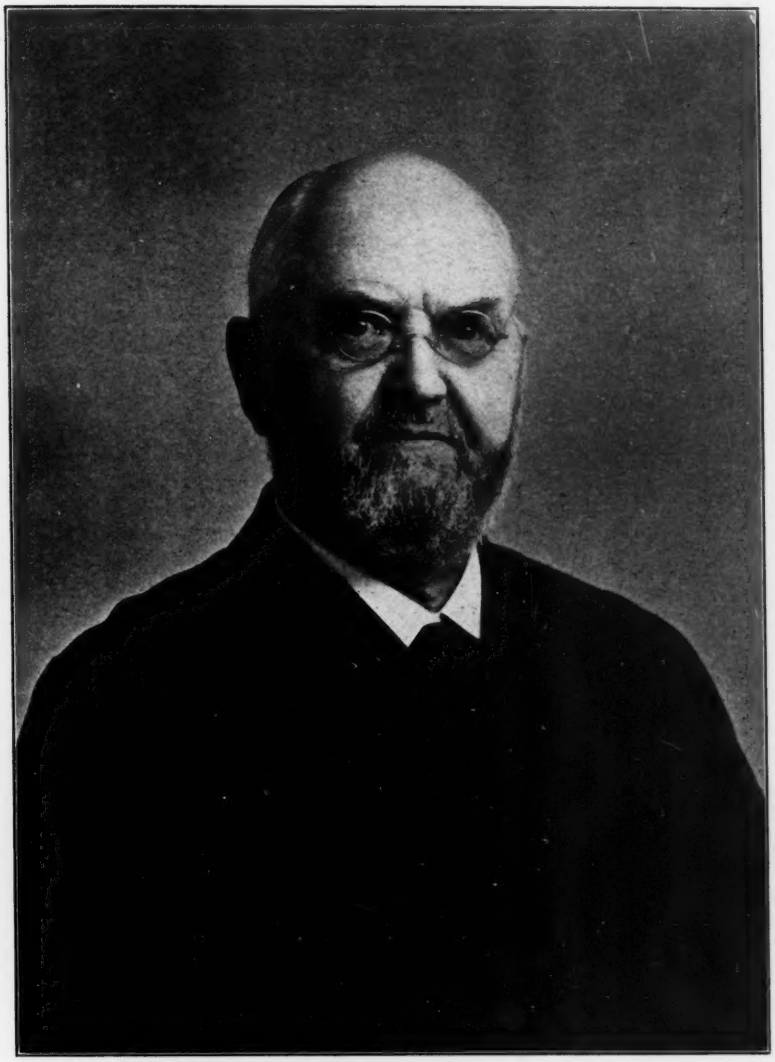
GEORGE FREDERICK BARKER.

803,501, an increase of a million and a quarter in a single year, and probably a larger number of persons than visited any other institution in the world for scientific entertainment and instruction. There were no increases in the collections, as there is no room for them. The director very properly urges the desirability of enlarging the aquarium and providing laboratories for scientific work and men to carry it forward.

DEATHS AMONG AMERICAN MEN OF SCIENCE

SINCE the death of Mr. Alexander Agassiz, in April, we have lost three other American scientific men officially placed among the hundred who are most eminent by their membership in the National Academy of Sciences. They are Professor George Frederick Barker, General Cyrus Ballou Comstock and Dr. Charles Abiathar White.

Professor Barker, who was both a



CHARLES ABIATHAR WHITE.

chemist and a physicist, was born in 1835 and graduated from Yale in 1858 and later in medicine from the Albany Medical School. He held various positions, including the chair of physiological chemistry at Yale until 1873, when he became professor of physics at the University of Pennsylvania, and for thirty-seven years, latterly as professor emeritus, held a leading position in the university, when Philadelphia had a more dominant position in science than it has been able to maintain. Professor Barker was an admirable lecturer and the author of widely-used text-books of chemistry and physics; he served as expert in important legal cases and carried forward research work of consequence. He was elected to the National Academy in 1876 and was president of the American Association for the Advancement of Science in 1879.

General Comstock, born in 1831, graduated from West Point in 1855 and taught physics in the academy.

He was actively engaged in the civil war, first in the defenses of Washington and later as chief engineer and senior aide-de-camp to General Grant. Later he became superintendent of the geodetic survey of the great lakes and of the improvements at the mouth of the Mississippi, and published works on these and other engineering topics. He was elected to the National Academy in 1884, and in 1907 gave the academy a fund of $10,000 for the promotion of researches in electricity magnetism and radian energy.

Charles Abiathar White, born in 1826, though early interested in science, was late in beginning professorial work. He received a degree in medicine at the age of thirty-seven and three years later became state geologist of Iowa and professor of natural history in the state university. He accepted a chair in Bowdoin College in 1873 and two years later became geologist in the surveys of Powell and Hayden. For many years he was connected with the Geological Survey, the National Museum and the Smithsonian Institution. He was elected to the National Academy in 1889. He published over two hundred contributions to geology, zoology and botany, maintaining his scientific activity to the end, as is indicated by an article in a recent volume of this journal.

Mr. Agassiz and Professor Barker died at the age of seventy-five, General Constock at the age of seventy-nine, Dr. White at the age of eighty-five. Another American scientific man who played an important part during the second half of the last century and died with his life work fully accomplished was Professor William Phipps Blake. He was born in 1826 and made valuable studies in the mineral deposits and geological structure of the Rocky Mountain and Pacific coast regions. Dr. Amos Emerson Dolbear, for thirty-six years professor of physics at Tufts College, known for inventions and other work in physical science, has died at the age of seventy-three years. Professor Robert Parr Whitfield, of the American Museum of Natural History, eminent as a geologist, has died at the age of eighty-two years. Dr. Cyrus Thomas, archeologist in the Bureau of American Ethnology since 1882, well known for his contributions to anthropology, has died at the age of eighty-five years.

More grievous than the death of veteran men of science is the loss of those whose work is not accomplished. Charles Reid Barnes, professor of plant pathology in the University of Chicago, dying after a fall at the age of fifty-two, was among our leaders in botany in both performance and promise. Dr. H. T. Ricketts, also of the University of Chicago, but called to the University of Pennsylvania, died in Mexico City at the age of thirty-nine years from typhus fever contracted as a result of research work on that disease. Even this partial list shows how severe have been the losses by death from among American men of science during the past six months.

SCIENTIFIC ITEMS

THE Paris Academy of Sciences has conferred the Janssen Prize, consisting of a gold medal, on Director W. W. Campbell, of the Lick Observatory.—Professor Theodore W. Richards, of Harvard University, has been invited by the Chemical Society (London) to deliver the next Faraday lecture. This will be the tenth Faraday lecture, the others having been given as follows: Dumas, 1869; Cannizzaro, 1872; Hofmann, 1875; Wurtz, 1879; Helmholtz, 1881; Mendeléef, 1889; Rayleigh, 1895; Ostwald, 1904; Emil Fischer, 1907.—Dr. John Benjamin Murphy, professor of surgery in Northwestern University, has been elected president of the American Medical Association, for the meeting to be held next year at Los Angeles.